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THE INTERNATIONAL CONGRESS OF ARTS AND SCIENCE.

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THE first International Congress of Arts and Science has passed honorably into history. What may have been a philosopher's dream is now also a fact accomplished. Not that with the successful completion of the program the living influence of the congress has ceased. Rather, indeed, is it certain to continue and possibly to yield increase beyond foretelling. This is really implied in the statement that the undertaking was a success, as must appear to all who are cognizant of the unique purpose and the correspondingly definite plan of the whole. With this our readers may be assumed to be in a general way familiar. In this article some attempt will be made to sketch, unhappily in an all too fragmentary fashion, the actual operation and course of the congress and to indicate, in a manner necessarily inadequate, a few tentative impressions as to its outcome and probable value. No single man, least of all one who had the pleasure of attending the congress in blissful ignorance of the reporter's task which was in store for him, can hope to do justice to a program so vast and so varied as was that which filled the week from the nineteenth to the twenty-fifth of September last at St. Louis.

The great exposition now in progress is notable not only for its material illustration of the arts and industries of the world, but chiefly because in its conception the place of first importance has been given to education. This means the explicit acknowledgment of the sovereignty of mind in human progress to a degree unprecedented in similar undertakings. It was therefore peculiarly fitting that the management should make a special effort to assemble a congress of the world's leaders in the acquisition, elaboration and application of knowledge, as a worthy spiritual capstone to the magnificent material edifice

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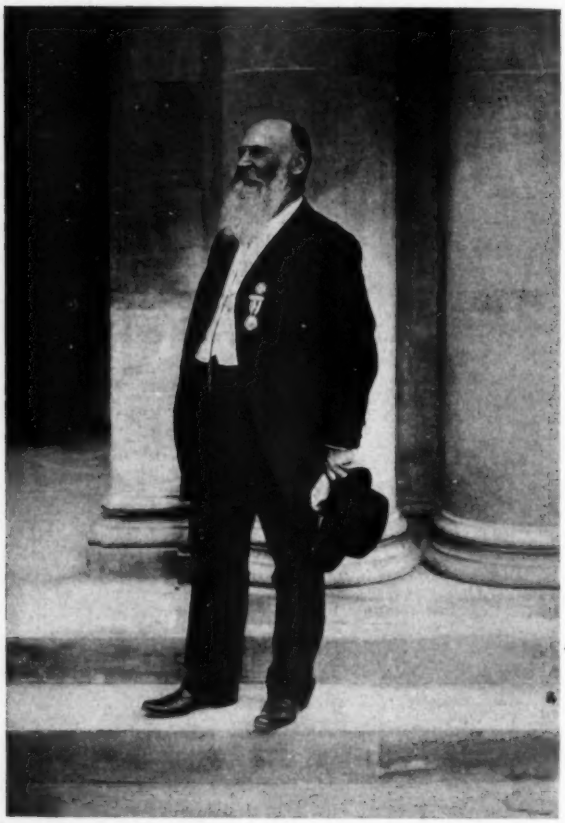


PROFESSOR WILHELM WALDEYER, of the University of Berlin, Honorary Vice-president of the Congress for Germany, who gave the address on Human Anatomy. The portraits illustrating this article were taken for THE POPULAR SCIENCE MONTHLY by Mrs. Jessie Tarbox Beals, Press Photographer, St. Louis, Mo.

which their enterprise had erected. Imposing as is the mere array of the visible tokens of progress in material civilization—a progress born of science, nourished by science, and in its turn begetting science—the whole must appear considerably more impressive when seen to be, as it really is, the outer expression of the inner intellectual life of mankind, the index of its vigor and plenitude, and the earnest of its future possibilities. Almost all the departmental exhibits, including manufactures, machinery, electricity, transportation, agriculture, horticulture, forestry, mines and metallurgy, and the like, illustrate directly the progress of applied science, while all, without exception, depend for their existence upon its development. Government and social economy presuppose some kind of philosophy and even a certain amount of knowledge, and both will increasingly apply the methods and results of science. Even the fine arts, quite apart from their technique, appeal to the reason and depend upon criticism. Some of the exhibits

were strictly in pure science, as *e. g.*, the department of anthropology, which included a laboratory of anthropometry and psychometry.

Such considerations suggest the ideal advantage which the Congress of Arts and Science enjoyed through its relation to the great exposition. It would further seem a happy thought which led to the convocation of an international group of scholars at a place independently dedicated as a meeting ground of the nations. Not to mention other advantages of a more practical kind for such of the participants as were interested in seeing a great world's fair, or studying some special aspect of its exhibition, the provision for this congress was at once a tribute to science many times deserved, and especially a lesson to the public at large of incalculable educational value. Besides, here was an opportunity too rare to be despised, of realizing, however imperfectly, a worthy ambition, widely shared, for the internal improvement of the whole kingdom of knowledge.

On the other hand, there were obvious drawbacks to the satisfactory conduct of meetings for the serious discussion of abstract and learned subjects under the conditions presented by a world's fair. It was impossible for the committee to overcome all the difficulties incidental to the subordination of the congress to the management of the exposition, of which it was externally but a small part, however significant. The fair's department of congresses had to provide for at least one hundred and fifty special conventions or international congresses of one kind or another, besides this universal congress. It was unfortunate that more halls suitable for speaking and hearing, and less widely scattered, could not be found or spared, and that no proper waiting and lounging room was provided for social intercourse. Yet great scholars spoke cheerfully to attentive listeners and congenial spirits contrived to meet for friendly conversation or for seeing the fair together. If some of the foreign guests suffered temporary inconvenience on the score of the creature comforts, they will probably not long remember it against us.

Those acquainted with the conditions may well pause for wonder at the smoothness with which so complicated a piece of machinery was kept running, involving as it did the direction of so large and variegated a group of markedly individual men who were for the most part hurried. In this connection credit is especially due the energy, patience and industry of the faithful staff of executive assistants under the efficient direction of the executive secretary of the congress, Dr. L. O. Howard, of Washington, government entomologist and chief of the division of entomology, U. S. department of agriculture. The congress was fortunate in securing for this arduous work the services of so eminent a worker for science, both as an investigator and as the permanent secretary of the American Association for the Advancement of Science, with his able assistant, Mr. Clifton. It is significant of the spirit of the congress that Dr. Howard's special executive assist-

ants were six doctors of philosophy engaged in university teaching, who also served as secretaries for their special sectional meetings.

This was a congress of scholars, conceived, administered and conducted by scholars. Under the general supervision of Dr. Howard J. Rogers, deputy superintendent of education for the state of New York, who was the official director of congresses for the exposition, it was arranged by an administrative board consisting of Presidents Harper of Chicago, Jesse of Missouri, Pritchett of the Massachusetts Institute of Technology, Librarian of Congress Putnam and Director Skiff of the Field Columbian Museum, under the chairmanship of President Butler of Columbia University, a professor of philosophy and a distinguished expert in education.

The congress was presided over by Simon Newcomb, retired professor in the United States Navy, a profoundly original, accomplished and productive astronomer and mathematician, perhaps of all Americans the most honored throughout the world among the peers of the realm of science. Hugo Münsterberg, Harvard's brilliant psychologist, philosopher and man of letters, author of the original plan of the congress, to whom is also due the largest share of credit for the splendid achievement, divided the vice-presidency with Albion W. Small, professor of sociology in the University of Chicago, whose wide grasp of the multifarious activities of organized social regulation and culture is largely responsible for their elaborate representation in the final scheme.

Thanks to the wisdom, enthusiasm and devotion of these learned officers of the congress, and the intelligent enterprise of the management of the exposition under its president, the Hon. David R. Francis, it was possible to carry out a program without a parallel in history. Other meetings of men, including some more eminent than any of those who came to St. Louis, may have accomplished more for science and civilization, but never before has there been a gathering of so large and representative a body of the world's leading scholars and thinkers. It will be sufficient to remind our readers in passing that the unique purpose of this congress was to see science whole. It was a deliberate attempt to exhibit the totality of intellectual achievement, to formulate the interrelations of the several branches of knowledge, and in some measure to realize the potential unity of the several sciences and their applications by harmonizing the confused mass of knowledge scattered through a bewildering multiplicity of specialties.

The program was designed to exhibit a certain dramatic unity in the order of the proceedings, which represented a progressive differentiation from the most general treatment of knowledge in the opening session through the several divisional and departmental addresses to the more specialistic sectional discussions, with a view to effecting an ultimate integration within each group, from the particular sections up to the all-inclusive whole of knowledge. The manner of the actual acting out of this ambitious plot during that memorable week in Sep-

tember must now be sketched in broad outlines and the details of the performance indicated by the barest reference to a few cases which may be taken as typical. Here the writer, lacking an adequate grasp of the whole, must be limited for illustration to the parts of the program with which he happens to be somewhat familiar.

Most of the members of the congress reached St. Louis on the morning of Monday, the nineteenth, registering in the exposition's Hall of Congresses, reserved as the official headquarters. The foreign guests were largely accommodated within the grounds in the dormitories of Washington University. The expenses of all the principal

PICARD. MOORE. MASCHKE.

Professor E. H. Moore of the University of Chicago presided, and M. Emile Picard of the Sorbonne and Professor H. Maschke of the University of Chicago gave the Addresses before the Section of Algebra and Analysis.

CLARKE. JORDAN. OSBORN. SCOTT. WOODWARD.

Officers of the Section of Paleontology and President David Starr Jordan, who gave the address on the Utilitarian Sciences. The addresses on Paleontology were given by Professor Henry F. Osborn, of Columbia University, and Dr. A. S. Woodward, of the British Museum. Professor W. B. Scott, of Princeton University, was Chairman and Dr. John M. Clarke, Geologist of the State of New York, was Secretary of the Section.

speakers were liberally paid and each had been asked to prepare an address on a prescribed topic having a definite place in the total scheme.

The officers and speakers numbered over 500, while the total registration was about 2,000. Many probably attended meetings and enjoyed the privileges of the congress without registering as regular participants. Almost all the leading colleges, universities and higher institutions of learning at home and abroad were represented, although about 125 officers and speakers were scholars not engaged in university teaching. These include museum curators, experts in government scientific bureaus, representatives of research institutions, observatories, the army and navy, legislative bodies and diplomatic embassies, besides eminent municipal officers, school administrators, editors, librarians, engineers, architects, artists, physicians, social workers, clergymen, lawyers and jurists.

About 90 of the speakers were foreign scholars, of whom perhaps 60 addressed the congress in their own languages—German, French, Italian or even Dutch.

Counting the Mexican and the half dozen Canadians for America, there remain about 25 from Great Britain, over 30 from Germany, almost 20 from France, a half dozen from Austria, about as many from Italy, at least 4 from Japan, one from Russia, and about 10 representing other countries—Holland, Belgium, Denmark, Sweden, Switzerland and Hungary. The Americans came from all parts of the country, a meritorious group of scholars worthy to receive their eminent guests from abroad. No less than seven women, distinguished for scholarship, were among the Americans who addressed the congress.

It was indeed a notable audience of men and women which assembled in the great Festival Hall of the exposition for the formal opening on Monday afternoon. Here under one roof were seated hundreds of scholars brought together by the common interests of learning and research, come together to exchange ideas and to meet and hear their peers and leaders. On the platform sat the administrative officers of the congress, the president and vice-presidents, and a distinguished group of representative leaders in the science of foreign countries, who had been invited to act as honorary vice-presidents of the congress. The meeting was called to order by Director Rogers, who called upon President Francis of the exposition to preside over the preliminary part of the session. In an appropriate address of welcome the congress was declared by President Francis to be the crowning feature of the exposition. An address was then made by Director Skiff, in charge of the exhibits of the exposition, on behalf of the administrative board, in which the exposition was characterized as a world's university, the exhibits being its museum and laboratories, and the participants of the congress its faculty. In the regretted absence of President Butler, made necessary by serious illness in his family, President Harper made a suitable address, setting forth the history of the plan and the prepa-

rations for the work of the congress. Responses followed by the honorary vice-presidents. England was represented, in the absence of Mr. Bryce, by Sir William Ramsay, K.C.B., professor in the Royal Institution of London, who stands in the forefront among inorganic chemists, distinguished by his discovery of argon and several other new chemical elements. M. Gaston Darboux, perpetual secretary of the Paris Academy of Sciences, one of the most original and profound inquirers in the field of modern geometry, spoke for France in the French language. Germany's spokesman was the venerable and venerated anatomist, the eminent Professor Wilhelm Waldeyer of the University of Berlin, who spoke in the language in which he has addressed two generations of medical students who have become leaders in the science of their profession. Dr. Oskar Backlund, director of the Russian Observatory at Pulkowa, which he has made famous by his celebrated astronomical measurements, expressed the greetings of Russia in English. Professor Theodor Escherich, the renowned Viennese pediatrician, spoke in German on behalf of Austria. Senior Attilio Brunialti, member of parliament and councilor of state at Rome, eminent student of constitutional law, which he came to discuss before the congress under the department of jurisprudence, after a few preliminary words in English, broke into his own familiar tongue in order, as he explained, to do justice to the feelings by which he was moved. The demonstrative enthusiasm of his expression was reciprocated by the hearty applause of the audience. In this succession of striking addresses by eminent personages, so individual and at the same time so representative, the keynote of the congress was struck and the spirit which animated the whole was quickly caught.

The scientific part of the program was then set going by the introductory address of the president of the congress, Professor Newcomb, who sketched in a scholarly and illuminating manner the evolution of the scientific investigator, who must be regarded as 'the primary agent in the movement which has elevated man to the masterful position he now occupies.' The common motives of all research and the vital significance of all truth for civilization and human welfare, as brought out in this address, seemed at once to suggest and to justify the universal scope and synthetic purpose of the congress, which was to comprehend not only all the branches of theoretic knowledge, but their several applications to the arts of life.

On Tuesday morning the seven grand divisions of the congress convened simultaneously, each division being addressed by an American scholar, chosen because of a conspicuous breadth of grasp in a wide domain of inquiry and an authoritative appreciation of its inner unity.

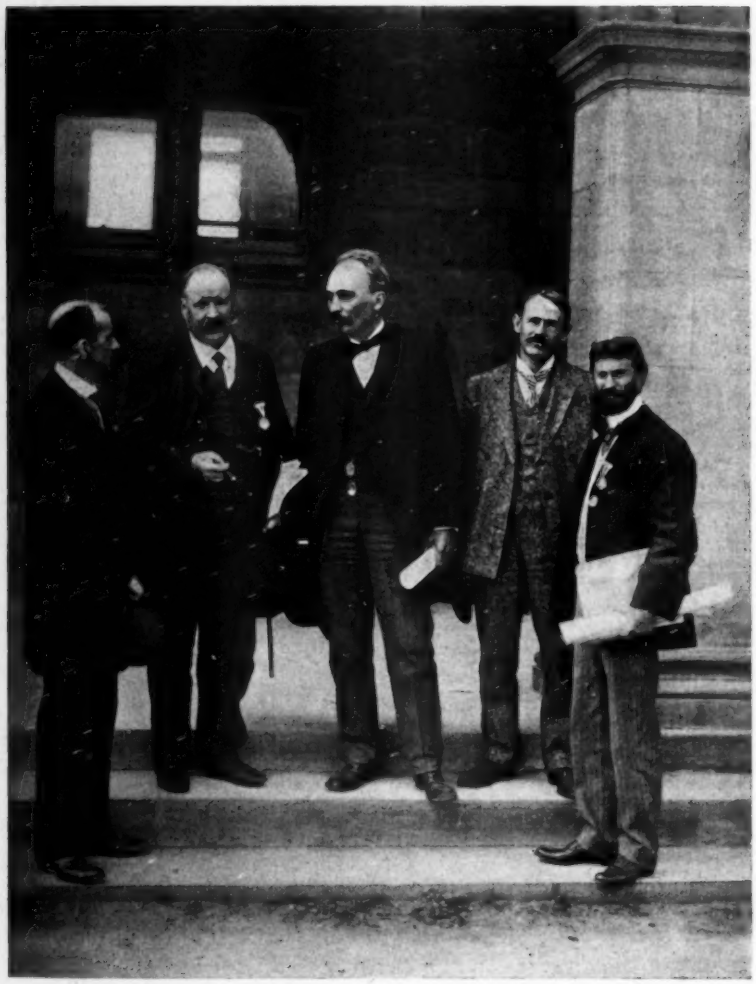
Thus normative science, inclusive of philosophy and mathematics in their entirety, was discussed by Professor Josiah Royce of Harvard University, America's foremost representative of speculative philosophy, unrivaled in systematic and constructive learning, unexceled in subtlety

of insight, who has awakened a fruitful interest in mathematics among philosophers and in philosophy among mathematicians.

In a remarkable address, too technical to summarize here, in which, to the luminous exposition of old ideas, was added the suggestion of novel ideas of fundamental interest, a statement was made of the community of interest shared by the several philosophical and mathematical sciences when abstractly regarded, and an account given of certain concrete investigations typical of the contemporary mutuality of interest among the more advanced students of philosophy and mathematics, together with some promising results. The sciences here grouped together as normative all agree in that they seek 'ideal' truth, as distinct from physical truth or from historical fact—are concerned with the consequences, implications and interrelations of ideas or of ideals, rather than with the order of phenomena or events. The mathematician is concerned with the exact expression and abstract logical development of ideas, the meaning of which in terms of their ultimate relations is sought by the philosopher. Both groups of sciences in all their branches are in need of a theory of the 'categories' or the fundamental and logically elementary conceptions by means of which human minds think; and in the discovery of such categories and their critical classification students in both groups must cooperate.

The discussions under physical science, embracing not only physics, chemistry, astronomy and the sciences of the earth, but biology and anthropology as well, were heralded by Dr. Robert S. Woodward, professor of mechanics and dean of the school of pure science at Columbia, distinguished alike for his buoyant efficiency and for his skillful command of the mathematics as a tool of physical inquiry, in which he combines a conspicuous catholicity in scholarship with a rare versatility in research, having been especially successful in the treatment of problems in cosmical mechanics which overlap the borders of many sciences. Professor Woodward pointed out a threefold unity in all physical science—a unity of origin in observation and experiment, a unity of growth in quantitative expression and elaboration toward prediction as a goal, and a unity of purpose in its attempt to describe the universe in 'consistent and verifiable terms.' A culminating unity, linking physical science to all other science, may be found in the light which it throws on man, and the human ends which it fulfills.

The unity and variety of historical science, comprising political and economic history, and the histories of law, language, literature, art and religion, was discussed with characteristic literary distinction by Woodrow Wilson, president of Princeton University, widely known as an historical student of politics, a literary student of history, an engaging cultivator of literature and a fond admirer of all the humanities. The conditions requisite to a needed synthesis both in the teaching and the writing of history were pointed out, with special emphasis upon the services of literary art and the conceiving imagination. The



ROTCH. ARRHENIUS. NIPHER. CLAYTON. BAUER.

After the Adjournment of the Section of Cosmical Physics. The Addresses were given by Professor Svante Arrhenius, of the University of Stockholm, and Dr. A. L. Rotch, Director of the Blue Hill Meteorological Observatory. Dr. L. A. Bauer, of Washington, was Secretary.

historian of religion was said to be especially concerned with the workings of an incalculable supernatural factor, which indeed no history can wisely ignore.

The spirit of mental science, subsumed under which were psychology and sociology, was voiced by Dr. G. Stanley Hall, an eager student of human life, who has achieved an encyclopedic learning; the creator of a school in the investigation of problems in mental science conceived and approached in a singularly generous spirit, who has made Clark University, of which he has been president since it was founded, preeminently an institution for psychological research; while, as teacher, editor and organizer, he has inspired an enthusiasm for the

inductive study of pedagogical problems which has made itself felt throughout the schools of the land. While Dr. Hall is one of few men who might have appropriately represented more than one of the great divisions of the congress, he chose on this occasion to discuss particularly the leading problems and methods of psychology, in an address spoken from the heart, abounding in fertile suggestions and no less characteristically teeming with allusions—an address which insisted on the continuity if not the identity of life and mind, and emphasized the urgent need of an objective study, at once comprehensive and thorough, of every concrete phase of experience in all its heterogeneous richness, as a basis for subsequent generalization under the guidance of the principle of evolution.

The division of utilitarian sciences, giving shelter to medicine, technology and economics, was generalled by President David Starr Jordan, of Leland Stanford University, eminent as a systematic zoologist, especially in ichthyology, who has placed his special knowledge at the service of the state in relation to questions of international import, and has admirably exemplified in his career what, as educator, writer and publicist, he has enunciated in no uncertain terms—the union of theory with practice in intelligent, effective work. President Jordan pointed out the unity of all the so-called utilitarian sciences in that they have their common source in disinterested investigation, at the same time claiming for pure research and practical application a relation of reciprocal dependence. Science is the guide of life and pure science must precede its applications, yet the utilities of science may not only determine the direction of its efforts but must ultimately control its results, measuring their exactness by the relentless standard of consequences.

The general interests of social regulation—in the sphere of politics, of jurisprudence and of those human groups with the economy of which social science is concerned—were entrusted to Mr. A. Lawrence Lowell, a distingushed member of a distinguished American family, professor of civil government in Harvard University, a scientific student of legislation, who has brought to the examination of political and legal institutions a ripe scholarship and an exceptionally critical mind. It is reported that in Professor Lowell's address the discussion took a somewhat practical turn, emphasizing especially the many-sided race problem, which was considered both historically and in relation to present-day conditions.

It almost goes without saying that the choice of a spokesman for social culture, through the great agencies of education and religion, fell upon the Honorable William T. Harris, U. S. Commissioner of Education, an alert survivor of the transcendentalist movement in America, celebrated for his learned familiarity with the history of civilization no less than for his indefatigable acuteness in the speculative interpretation of its principles after the manner of a philosophy

Before the Addresses of Professors Ostwald and Erdmann on the Methodology of Science.

which has exerted a profound influence alike upon educational theory and upon religious thought, and withal a practical student and expert adviser in conspicuously active touch with the complex organization of instruction in our schools. Commissioner Harris doubtless traced the history of social culture from its first beginnings, as the unfolding temporal expression of an immanent purpose realizing itself through the instrumentality of human institutions, and having therefore from the start a unity of aim, at once natural and divine, namely, the perfection of spiritual citizenship in a rational society of personal selves.

Thus it was, in a general way, that these 'seven wise men' opened the gates of their respective fields of science. It is unfortunate that the aspiring listener was limited to the choice of a single one of this series. These men and their discourses have been chosen for specific characterization because they furnish a clue to the diversities as well as to the unities which pervaded the whole of the congress.

The divisional addresses over, the twenty-four departments were free to open fire. These were operated, eight at a time, at three intervals, scheduled for the late mornings and for the early and the late afternoons, respectively. It was thus possible for, say, the philosopher to do his duty by his own department at 11.15 while enjoying that of psychology at 2, with the freedom after 4 to choose between education, religion, sociology or some department farther afield, or yet again, to see something of the fair!

No account of the departments can be given which would be at all representative. Their titles have been already indicated under each of the divisions. It should be recorded that in general the departmental meetings were conducted by Americans, one in the chair and two with prepared addresses respectively on fundamental conceptions and methods, and on the history of progress during the last century. In some cases the fundamental character of the conceptions and methods discussed might be opened to question, and in others the ancient habit of beginning any history with Adam was not successfully inhibited, yet on the whole these departmental discourses did conform to the specifications prescribed, and the two addresses nicely supplemented each other. It remains only to illustrate the *personnel* a little more fully.

Taking philosophy as the first on the program, its chairman was Professor Borden P. Bowne, of Boston University, valued for his conservative temper, constructive scholarship, and the keen, clear and interesting analysis long familiar to a group of grateful students and readers. The historical paper was presented by Professor George Trumbull Ladd, celebrated for the comprehensiveness and thoroughness of his productive erudition over the whole field of mental and moral philosophy and as one who helped to lay the cornerstone of experimental psychology in America, whose name is honorably associated with

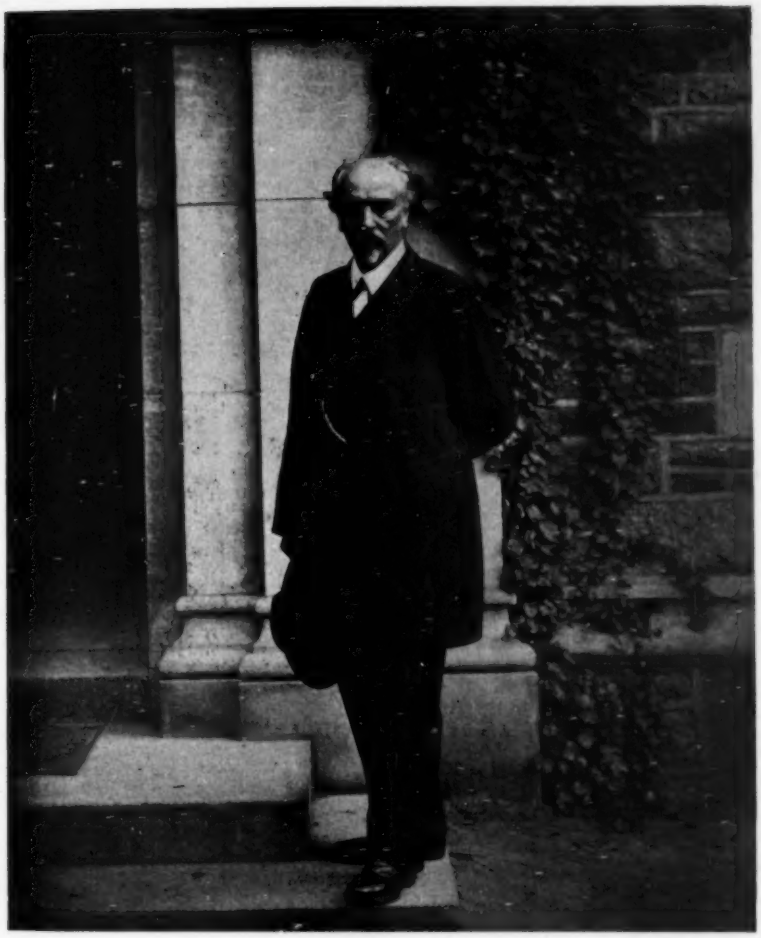
the department at Yale University, where he early founded a psychological laboratory which has taken high rank among like establishments throughout the world. The other paper for philosophy, read in the author's absence by Professor Charles M. Gayley, of the University of California, had been prepared by Professor George H. Howison, of that university, a man beloved and revered as an inspiring teacher of philosophy by a large and able body of students scattered over a continent, uniting intense moral fervor with a highly developed metaphysical imagination, and acknowledged as the most consistent defender of 'spiritual idealism' in academic circles.

Since it is impossible to mention by name all of the departmental speakers, let us glance at the list for the physical and mental sciences, sometimes called 'descriptive.' Nichols and Barus, Nef and Clarke, Pickering and Boss, Davis and Chamberlin, Loeb and Coulter, McGee and Boas, Baldwin and Cattell, Vincent and Giddings—of such names we need not be ashamed.

The important work of the 128 sections began on Wednesday and lasted through Saturday, the two sections in religious influence postponing their sessions till Sunday. Each sectional meeting occupied the greater part of a morning or an afternoon.

The offices of chairman and secretary for each of the sections were filled by Americans, chairmen being for the most part specialists of eminence, while the secretaries usually, although by no means invariably, represented a younger generation of scholars, conspicuous for promise.

To take the first group of sections, under philosophy, the chairmen were Professors Armstrong, of Wesleyan, for metaphysics; Thomas Hall, of Union Theological Seminary, for philosophy of religion; Duncan, of Yale, for logic; Creighton, of Cornell, for methodology of science; Palmer, of Harvard, for ethics, and Tufts, of Chicago, for esthetics. The secretaries, named in the same order, were Professor A. L. Lovejoy, of Washington University; Dr. W. P. Montague, of Columbia; Dr. W. H. Sheldon, of Columbia; Dr. Ralph Barton Perry, of Harvard; Professor F. C. Sharp, of Wisconsin; and Professor Max Meyer, of Missouri. The two principal speakers were supposed to treat one of the relations of their special science to neighboring sciences, in the interests of orientation and adjustment, the other of present problems demanding investigation in the immediate future. Many of the sections listened in addition to one or more ten-minute papers, which showed a tendency toward a general treatment of these topics harmonious with the principal addresses, although some were very special, in no sense intended to complete or supplement the main discussion. Thus one interesting paper made use of a lantern to illustrate the morphology and development of the kidney tubule. Impromptu discussion was opened in the section after the delivery of the formal communications. The principal addresses were made in



DR. BENNO ERDMANN, Professor of Philosophy at the University of Bonn, who gave one of the Addresses on the Methodology of Science.

some cases by two foreigners, in others by two Americans, but in most of them by a foreigner and an American scholar in turn.

Philosophy must again serve for illustration. The problems and relations of metaphysics were grappled with by two live thinkers, both authors of important recent books of a strictly metaphysical nature. The first was Professor A. E. Taylor, of McGill University, reputed for his firm and clear grasp of fundamental metaphysical and ethical problems and an exceptional acuteness in analysis. The second was Alexander T. Ormond, Princeton's influential teacher of philosophy, known as one of the few profoundly constructive metaphysical teachers among Americans, and a veritable paragon of synthesis in philosophy.

The philosophy of religion was shared by two eminent Germans,

DR. WILHELM OSTWALD, Professor of Chemistry at the University of Leipzig, who gave one of the Addresses on the Methodology of Science.

representing somewhat different standpoints: Dr. Otto Pfleiderer, the distinguished Berlin professor, a comparative historian and speculative thinker of the neo-Kantian persuasion, in the domain of religion, conceived as a logical development of ideas in history, and Professor Ernst Troeltsch, of Heidelberg, a philosopher to be sure, but eminent primarily as scientific student and critic of literary documents and historical sources in religion. Pfleiderer read in English, Troeltsch in German.

Two Americans spoke for logic, Professor Wm. A. Hammond, of Cornell, esteemed as a careful student of logic, metaphysics and psychology, especially in their historical aspects, and one of the few trained American scholars who has given serious attention to Greek philosophy, and Professor Frederick J. E. Woodbridge, of Columbia, who has made a marked impression as a scholarly teacher of philosophy, alike for originality and independence in the interpretation of philosophical systems and for the freshness which he has infused into metaphysical problems, while insisting on their intimate correlation with the problems of logic, in terms of which he has boldly defined them.

The session for the methodology of science was one of the most characteristic features of the whole congress. The subject itself is both new and fundamental, and perfectly typical of the class of problems for the consideration of which the congress was planned. It has to do with the determination and mutual adjustment of the concepts which underlie the special sciences and the methods peculiar to each, and is thus in the closest relation to logic and to the sciences themselves. A science of method must both conform to the laws of our minds and apply to the subject matter of experience which the sciences severally study. It was therefore essential that both speakers should be at the same time philosophers and men of science. And it was no less fitting than interesting that they should approach their subject by different roads. One is primarily a physical scientist, the other primarily a philosopher; both are preeminent. The first was Wilhelm Ostwald, of Leipzig, one of the most interesting personalities among living men of science. A brilliant investigator in the field of physical chemistry, where his name is linked with those of van't Hoff, Arrhenius and Nernst; a great teacher of chemistry and author of a monumental systematic treatise therein, conceived in a spirit original and unique; an ingenious expositor of the new doctrine of energetics in physical science; an enthusiastic student of philosophy, who has played up and down the whole gamut of the sciences; recently the founder of a new journal of natural philosophy, which is the acknowledged organ of a flourishing school:—such a man is Ostwald—a kind of 'modern Siegfried,' as an eminent colleague put it. The second speaker, also a German—

'He of the twice illustrious name,'

Benno Erdmann, of Bonn, a man profoundly learned in historical philosophy, who has advanced by important contributions, not unrelated, two such different sciences as experimental psychology and modern logic, in which he is an acknowledged master.

This session promised well from the start. The audience was representative in more ways than one. In addition to the professional philosophers, a number of remarkable men of science were present. In the front row, for instance, sat Svante Arrhenius, of Stockholm, physical chemist, famous for his mathematical and experimental contributions to the theory of solutions and speaker for cosmical physics, and Ludwig Boltzmann, of Vienna, mathematical physicist, distinguished especially for his work in the kinetic theory of gases, who represented applied mathematics in the program of the congress. Scattered through the audience were many eminent leading representatives of both the physical and mental sciences. Among Americans on the front were Loeb in biology and Cattell in psychology, both eminent specialists actively interested in scientific methods, both having applied exact methods with conspicuous success, albeit in very different ways, to the investigation of phenomena of life and mind. This is not the place to attempt a summary of the leading addresses, both delivered in German. Suffice it to point out that Ostwald presented a classification of the sciences professedly based on the empirical standpoint of energetics, and bearing but slight resemblance to the elaborate scheme which shaped the program of the congress. Erdmann attacked the subject from a frankly *à priori* point of view, arguing for the position which has recourse to a generating principle of logical necessity. Among those who took part in the open discussion were Boltzmann, Hoeffding, Ormond, Miss Calkins, etc., and the chief speakers again in reply.

In the section for ethics the speakers were Professor William R. Sorley, of Cambridge, able philosophical moralist, keen critic of the ethics of naturalism, who has made sound learning, astuteness and vigor of mind tell also in the study of the more vital questions of practice, and Professor Paul Hensel, of the University of Erlangen, philosophical scholar and critic, a gifted student of ethical theory and interpreter of ethical ideals in literature. The first address in the esthetics section was made by Dr. Henry Rutgers Marshall, of New York, a successful architect by profession, a philosopher by instinct and performance, distinguished as a philosophical student of psychology and a psychological student of esthetics, who has made interesting contributions to the analytical and genetic aspects of both sciences. The second speaker was Professor Max Dessoir, of the University of Berlin, erudite historian of German psychology in all its ramifications, himself a philosopher, psychologist and esthetician.

And so one might run through the long program, apart from a fortunate limitation of space and an unfortunate but inevitable igno-

rance. Without systematic attempt at characterization, let us swiftly glance at a few scattered sections. Thus E. H. Moore, of Chicago, presided over the section for algebra and analysis in which his colleague Heinrich Maschke spoke on the same platform with the illustrious Emile Picard, of the Sorbonne—a truly remarkable trio in the purest of pure sciences, the very problems of which are unknown variables even to the educated lay mind. Leave can not be taken of mathematics until attention is called to the immortal name of Poincaré, also of the Sorbonne, preeminent equally in pure mathematics and in their applications to physical research, especially to the problems of celestial mechanics, in which connection he has been called the 'Laplace of the present century.' Poincaré read his address before the section of applied mathematics.

Readers of the MONTHLY will be well familiar with the name of Karl Lamprecht, of Leipzig, great historian of Germanic culture and philosopher of history, who spoke without a note before the section of medieval history. Other sections of the same department were addressed by Mahaffy, of Dublin; Pais, of Naples; Cordier, of Paris; Bury, of Cambridge; Conrad, of Halle; while under the history of languages came MacDonell, of Oxford; Sonnenschein, of Birmingham; Jespersen, of Copenhagen; Paul Mayer and Lévi, of the Collège de France; and Sievers, of Leipzig, the highest authority in phonetics. Classical art was represented by Furtwängler, of Munich; modern painting by Muther, of Breslau, and the Japanese artist Okakura Kurozi, wearing native costume and attended by his lackeys. Sections devoted to the history of oriental religion heard Oldenberg, of Kiel; Goldziher, of Budapest; Budde, of Marburg; while the history of the Christian Church was discussed by that splendid historian Adolf Harnack, of Berlin, and the scarcely less distinguished Jean Reville, of the faculty of protestant theology of Paris, both of whom represented ecclesiastical history not as a thing apart, but as merely a distinguishable aspect within the continuous stream of civilization.

The physical sections are worthy of note for their threefold division into physics of matter, physics of ether, and physics of the electron. The last was discussed by Langevin, of the Collège de France, and the brilliant Rutherford, of McGill, whose experimental researches have resulted in the accepted theory of atomic distintegration as a cause of radioactivity. In one section Sir William Ramsay, in whose laboratory helium was first derived from radium, and the eminent French chemist, Henri Moissan, who, by the way, accomplished the manufacture of artificial diamonds in the laboratory, although his title to fame rests on a much broader foundation, discussed the science of inorganic chemistry. Physical chemistry was represented by the great van't Hoff, of Berlin, who developed the concept of osmotic pressure into a consistent theory of solutions and conceived the idea of

PROFESSOR K. MITSUKURI, OF THE UNIVERSITY OF TOKIO, WHO GAVE AN ADDRESS ON OCEANOGRAPHY.

the tetrahedral carbon atom in explanation of the behavior of substances chemically identical which nevertheless react differently to polarized light, thus creating the science of stereochemistry.

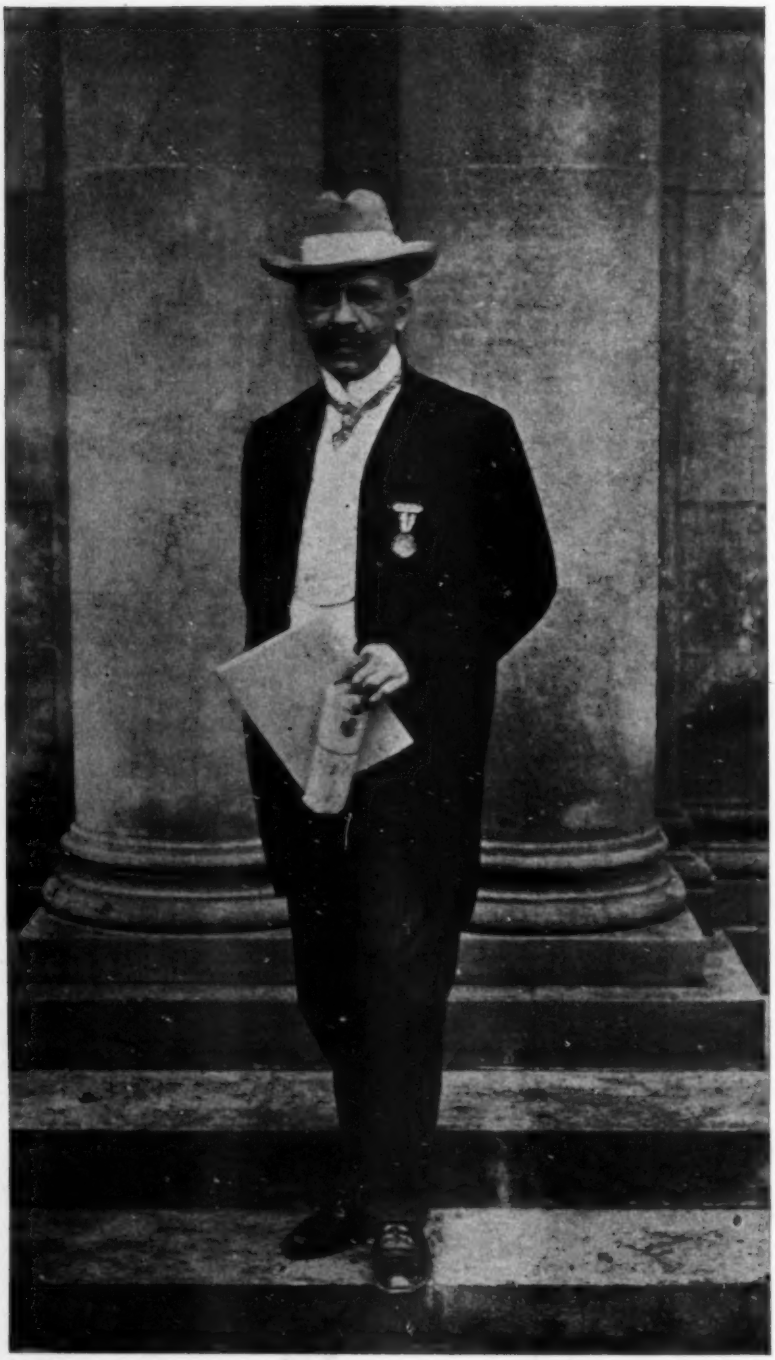
Besides Backlund, of Russia, the Dutch astronomer Kapteyn, of Groningen, celebrated for his measurements of stellar distances, discoursed on astronomy. Our own Campbell, of the Lick Observatory, delivered an admirable address on astrophysics, along with Turner, of Oxford. The section of paleontology, with Scott, of Princeton, in the chair, and John M. Clarke, New York

PROFESSOR JULIUS WIESNER, OF THE UNIVERSITY OF VIENNA, WHO GAVE AN ADDRESS ON PLANT PHYSIOLOGY.

state geologist, acting as secretary, was notable for its excellent address by Dr. A. Smith Woodward, F.R.S., the eminent systematic paleontologist of the British Museum of Natural History, and Professor Henry F. Osborn, of Columbia University and the American Museum of Natural History, together with the discussion which followed, of unusual interest, not only to paleontologists, but to geologists as well. Students of petrology and mineralogy heard Zirkel, of Leipzig, while those of physiography listened to Penck, of Vienna. The section of oceanography, manned by Rear-Admiral John R. Bartlett, U. S. N., in the chair, had the course of its deliberations directed by no less distinguished an explorer of the deep sea than Sir John Murray, K.C.B., of Edinburgh, aided by the eminent Professor K. Mitsukuri, of Tokio, a zoologist celebrated for his embryological researches and his knowledge of marine life in eastern waters.

The section of cosmical physics was another remarkable for the ideals of synthesis and the spirit of cooperation which pervaded it. In an address as bold as it was original Arrhenius proposed a theory of the possible connection between phenomena the most diverse and separated by exceedingly great distances, thus, *e. g.*, raising meteorology to the dignity of a cosmic science. Negatively charged electric corpuscles pass off from the sun and penetrate our atmosphere, producing its negative electricity, forming nuclei for the condensation of moisture, and so on, in intricate detail! Such students of meteorology and terrestrial magnetism as Drs. Rotch and Clayton, of the Blue Hill Observatory, and Dr. L. A. Bauer, of Washington, participated.

The biological sections were addressed by such eminent botanists from abroad as De Vries, of Amsterdam; Bower of Glasgow; Goebel, of Munich; Wiesner, of Vienna; and Drude, of Dresden; and by such representative zoologists as Giard, of Paris; Oskar Hertwig, of Berlin, recently made *rector magnificus* of the university; Delage, of Paris; Waldeyer, of Berlin; and Verworn, of Göttingen. Among the Americans who took part either as chairmen or speakers were Trelease and Bessey, Whitman, Brooks and Davenport, Meltzer, Howell and Theobald Smith. The section around which most interest centered was naturally that of phylogeny, presided over by T. H. Morgan, of Columbia, and addressed by De Vries and Whitman, one a botanist, the other a zoologist, both in the true sense biologists, who have directly investigated the problems of phylogeny and evolution by observation and experiment. De Vries, who has been in America since last spring, is professor in the University and director of the botanical gardens at Amsterdam, and eminent for a remarkable series of researches, experimental and theoretical, touching problems in physical chemistry and plant physiology, in the theory of heredity, and especially in the new experimental science of evolution. In the last named field De Vries has accomplished results which will make an epoch, at once demonstrating the fundamental thesis of Darwin, and supplementing the principles of Darwinism.



DR. HUGO MÜNSTERBERG, Professor of Psychology at Harvard University and Vice-President of the Congress, who proposed the Plan for the International Congress of Arts and Science.

For De Vries has been able to see with his own eyes the actual evolution of several new plant forms possessing the characters of true species, and has accumulated a vast amount of exact evidence, in support of the theory that new species arise suddenly from marked variations of the discontinuous sort, called 'mutations,' rather than by the gradual accumulation, through successive generations, of slight differences due to the ordinary 'fluctuating variation,' as Darwin had supposed.

This mutation theory of De Vries was discussed by its author before the section, while Whitman, after a general historical survey of his subject, discussed the interesting results obtained from a prolonged and controlled study of the evolution of color-pattern in the feathers of pigeons which he has bred for many years. Here the changes seem gradual, yet stable. As to the degree to which the two sets of results conflict, it would be premature to pronounce judgment.

Anthropologists were enabled to hear in their sectional meetings Manouvrier, of Paris, perhaps now the foremost name in physical anthropology; Seler, of Berlin, in American archeology; Haddon, of Cambridge, in ethnology.

The temptation must be resisted to report in detail the psychological sectional meetings. Denmark's ablest psychologist and England's, both eminent also in philosophy, discussed the relations and problems of general psychology with characteristic breadth and penetration. It was indeed a notable occasion when Hoeffding and Ward were introduced by Royce.

In another section Lloyd Morgan discussed the relations of the animal psychology which he may be said to have shaped for a band of younger workers who were in large part present to hear him, while Miss Mary W. Calkins, professor in Wellesley College, presented in excellent form a discriminating statement of the problems of genetic and comparative psychology in the large. As those addresses had been preceded by some well-chosen remarks by the chairman, Professor E. C. Sanford, who has directed important experimental work in comparative psychology, so they were followed by short papers of general methodological interest—one by the lamented Dr. C. L. Herrick, late editor of the *Journal of Comparative Neurology and Psychology*, read by Professor C. Judson Herrick, in which the dynamic or functional standpoint was emphasized; another by Dr. John B. Watson, of Chicago, urging the desirability of combining neurological studies, both experimental and histological, with systematic observations of animal behavior. Some matters of method, involving such questions as the criterion of consciousness, were broached in another short address, and there followed an interesting discussion which led to a pleasant lunch party.

The fascinating but baffling questions of abnormal psychology were discussed in another section by Dr. Pierre Janet, of the Salpêtrière, world-famed psychiatrist and psychologist, eminent as a

clinical investigator of abnormal conditions in the variegated and little understood field of the so-called psychic automatisms, obsessions, hysteria, 'multiple personality,' hypnotism, etc., whose penetrating analyses and fertile hypotheses have done much to bring unity and order into the chaos of phenomena presented. Dr. Morton Prince, an exceedingly clever Boston alienist, known also as a philosopher, offered an interesting array of facts from the field of the subconscious, which he subjected to an illuminating analysis. This section was particularly fortunate in having for its secretary an eminent alienist who has brought the methods and results of neurology and pathological anatomy, of physiology and psychology, together with clinical observation, to bear on a truly biological investigation of insanity—Dr. Adolf Meyer, of the New York State Pathological Institute.

One of the most interesting sessions was that for experimental psychology, in which a fundamental question of definition—far-reaching in its consequences for psychological research—was brought to a sharp issue in a fruitfully polemical address by Professor E. B. Titchener, Cornell's learned and thorough experimental psychologist, who has made a profound impression, not only upon a loyal group of students, but among psychologists everywhere, by reason of the distinctive point of view to which he has consistently adhered, no less than for the contagious enthusiasm of his devotion to the ideals of the experimental method. Titchener, after a masterly review of the present needs of experimental psychology, felt obliged to insist in sober earnest that psychology is in essence introspective, that introspection should be at the core of every psychological experiment, and that only those investigators who are concerned directly with conscious processes are properly psychologists at all, although it was conceded that much useful work—useful even for psychology *sensu stricto*—might be done by those who approach the subject more objectively, in the spirit of physiology or of biology, or, on the other side, from the standpoint of the theory of knowledge.

The interest attaching to the particular form which the discussion took before the experimental psychologists was enhanced by the fact that other psychologists had already, in their divisional and departmental addresses, favored the congress with their respective psychological creeds. For Hall, introspection was an almost anomalous by-product of evolution, for Cattell, only one of the methods of psychology. If Hall defined his science in terms of his general philosophy or *Weltanschauung,* and Titchener in terms of its most distinctive feature, Cattell may be said to have defined it inductively, in terms of the concrete interests of working psychologists as measured by their output. His was both a reasoned plea for a deliberate eclecticism in research, pending the adjustment of philosophical difficulties not easily banished, and a defense of a frank opportunism which has proved its usefulness. Ward's interests are apparently antipodal to Hall's. He

would agree with Titchener as to introspection, but in his address minimized the importance of sensations, and of just those simple, 'presented' aspects of experience which Titchener had emphasized as the most promising for study. Hall warned the psychologist against mathematics, while Cattell correlated psychology with the physical sciences and emphasized the need of exact methods, and Titchener found a large place for quantitative work. The psychology of Ward and Hoeffding seemed tenuous by contrast with that of all these others, while that of Janet and Prince occupied a place apart. Yet all were able discussions of psychology of some sort, and beneath the troubled surface was a common interest in 'minds,' many fathoms deep.

In the section for social structure there were three speakers, the noted Austrian field marshal, Gustav Ratzenhofer, of Vienna, the eminent social philosopher, Professor Toennies, of Kiel, and our own distinguished sociologist and paleobotanist, Lester F. Ward, of the U. S. National Museum.

One would despair of doing justice to the eminent representatives of the great and beneficent science of modern medicine, even if there were space for the attempt. Happily here, as perhaps generally in the case of the so-called utilitarian and other applied sciences, it will only be necessary to mention the names of a few of the leaders that addressed the congress to awaken appropriate associations in the reader's mind. For, most of the distinguished visitors who shared in the work of these sections enjoy, in addition to scientific eminence, a merited popular fame. Professor Ronald Ross, of the School of Tropical Medicine at Liverpool, whose name is a household word through his work on the rôle of the mosquito in the etiology of malarial fever, came for preventive medicine; Sir Lauder Brunton, of London, and Oscar Liebreich, of Berlin, for therapeutics and pharmacology; T. Clifford Allbutt, of Cambridge, for internal medicine; Sir Felix Semon, of London, for otology and laryngology; Theodor Escherich, of Vienna, for pediatrics; Shibasaburo Kitasato, of Tokio, bacteriologist and possibly Japan's most eminent man of science, for neurology. Many of our ablest American physicians and surgeons addressed the medical sections. In the only section which the writer was able to attend, that of psychiatry, after the excellent papers by Dr. Charles L. Dana, of New York, and Dr. Edward Cowles, of Boston, interesting remarks were made by several workers in neurology, psychiatry, and outlying fields, including such men as Janet, Hall, Ladd, Marshall, Prince, Meyer and Putnam.

The sections in technology, including the various branches of engineering, technical chemistry and agriculture, were conducted by prominent Americans, although the interest in this part of the program was scarcely commensurate with its importance. President Humphreys, of Stevens Institute, Professor Kennelly, of Harvard, Mr. John Hays Hammond, of New York, Professor Liberty H. Bailey, of

Cornell, and Hon. James Wilson, Secretary of Agriculture, were among the officers and speakers. The various branches of economics were discussed largely by Americans such as Clark in economic theory, Ripley in transportation, Seligman in public finance, and Hoffman in insurance, although Eugene von Philippovich came from Vienna.

In one of the political sections an address of exceptional interest was made by the Right Hon. James Bryce, M.P., eminent as a statesman, preeminent as a scholar in the field of political and legal history, gratefully honored by every educated American. The Hon. David Jayne Hill, our minister to Switzerland, spoke for diplomacy. Under jurisprudence, Professor La Fontaine, member of the Belgian Senate, spoke for international, and Signor Brunialti for constitutional, law. Professors Max Weber, Werner Sombart and T. Jastrow, and Dr. Emil Münsterberg, president of City Charities at Berlin, came from Germany alone for sections devoted to the social communities and groups. From abroad came Rein, of Jena, the eminent pedagogical philosopher, for educational theory; Michael E. Sadler, of Manchester, whose splendid work for public education in England has won the admiration of educators everywhere, for the section devoted to the school; M. Chabot, of Paris, for the university; and Guido Biagi, royal librarian at Florence, for the library. The section on the college listened to an address by that staunch and scholarly educator, President M. Carey Thomas, of Bryn Mawr College.

Among the speakers before the six sections concerned with practical religion were some who have exerted a wide popular influence, such as Rev. Hugh Black, of Edinburgh, Rabbi Hirsch, of Chicago, and Dr. Josiah Strong, of New York.

But the regular meetings were not enough. The Eighth International Geographic Congress, under the presidency of Commander Robert E. Peary, came to St. Louis to meet with the Congress of Arts and Science, and aroused considerable interest. Members of the congress having common technical interests were invited to special meetings of various sorts. Thus a Conference on Solar Research was held and an organization effected looking toward international cooperation among those interested in the investigation of solar problems. It is significant that almost all of the leading academies and other appropriate societies of the world which had been invited to cooperate, were ready with representatives from the membership of the congress.

Nor were the scientific meetings all. Men may be interesting though their theories be wrong. When they are known to have ideas and to have won distinction, they are especially interesting, even to those not technically familiar with their work. And while some scholars may seem neither to have inherited nor acquired the art of social enjoyment, the species is almost extinct, except in fiction and on the stage. There was entertainment enough, in varying degrees of informality, including the spontaneous formation of numerous

groups of manageable size for the investigation of 'The Pike.' No account of the congress would be complete which should fail to mention the series of formal entertainments arranged for its members. On Monday night the exposition celebrated the opening of the congress by special illuminations about the Grand Basin—a truly magnificent display. An attractive garden fête was given one afternoon at the French Pavilion by the Commissioner General from France. Another evening the German Imperial Commissioner General received at the German State House, with a hospitality that was handsome in its elegance and generosity. Other receptions were given by the Japanese Commissioner General and the Board of Lady Managers of the exposition. The Shaw banquet to the foreign delegates called forth numerous expressions of appreciation.

Perhaps in no event of the week was the informing spirit of the whole so impressively present to all as it was on the occasion of the closing banquet to the officers and speakers of the whole congress, tendered by the president of the exposition. In the great banquet hall of the Tyrolean Alps were assembled, for the second time, the whole *personnel* of the congress. The prevailing sentiment of scientific fellowship came out in all the speeches.

Commissioner Lewald made a ringing speech in German, Professor Darboux spoke in French, Signor Brunialti in Italian. Mr. Bryce, who spoke for British science with knowledge and with point, added that 'every meeting like this makes for international good will and every step like this is not only a step toward the advancement of knowledge: it is also a step toward the advancement of peace.' Notable too was the speech of Professor Nobushize Hozumi, of Tokio, one of the speakers in the section of comparative law and honorary vice-president for Japan. With winning felicity and consummate tact he expressed the pleasure which his countrymen had in cooperating with a distinguished Russian scholar in the congress, and added that this was the only place in which Japan could meet on equal terms that country with which it is at war in another part of the world.

The congress over, its members were soon scattered. Fortunately, many of the foreign guests were able to linger in our country for the purpose of traveling, visiting friends, or giving lectures. They were received by the President at the White House, also by Professor and Mrs. Newcomb in Washington; were entertained in Baltimore, Philadelphia, and elaborately in Boston and Cambridge by Professor Münsterberg and others; also at Yale and in New York, where the closing festivity was held under the auspices of the Association of Old German Students, and friendships old and new were cemented.

In what has preceded emphasis has been laid almost exclusively on the personal element. This it is which gave distinction to the congress and which made the most immediate impression. After the publication of the addresses, for which a special appropriation has

been made, it will be possible to study the ideas which were given expression in the meetings. Not until then will it be possible to estimate with safety the scientific outcome and value of the congress as a whole. It is certain, however, that the addresses were for the most part real contributions to science, and many of them of exceptional importance. It may strain the imagination of some to conceive of unification among subjects so diverse as logic and obstetrics, in spite of the Socratic simile. But it was the unity of ordered position in a complex system, and not without a sufficient number of intermediaries, that was sought, and in large measure realized. It would indeed have been a miracle if the sciences had simply been shaken together and a perfect kaleidoscopic picture had resulted. But such was not the case.

A unified classification had been prepared, as a means, not as an end, and elaborate as it was, it lent itself with remarkable fitness to the actual work of the congress. This does not mean that all the addresses conformed to the specifications in the same degree, or that those which heeded them most were always the most interesting. It is perhaps fair to say that if the dramatic unity of the whole was not mechanically perfect in its execution, it was ideally present throughout. Specialists were of course primarily interested in their own departments, but it was impossible not be conscious of the varied opulence of learning by which they were constantly surrounded and the one animating spirit of research with which the very atmosphere was surcharged. The leaders were mostly men whose previous interests and accomplishments were general and synthetic, as well as specialistic. Poincaré, Ostwald and Boltzmann might have been assigned to places in physical science; James Ward, to normative science; Arrhenius, to chemistry; Bryce, to history; while medicine would have been proud to open its doors to such savants as Waldeyer and Loeb.

A balanced evaluation of results must await the later work of better judges. Suffice it to remark in conclusion that a keen sentiment of mutual interest and respect was aroused and personal acquaintances were formed which should be an inspiration to all concerned. The congress will be a lasting monument to the idealism of the American spirit of enterprise. It gave a definite and a permanent expression to the scientific and social *Zeitgeist*. It must tend to quicken among scientific workers their sense of the multifarious variety of the human interests for which they labor, and to make for the desiderated extension of the methods of science to the whole domain of human life and effort, foreshadowing by the existing unity which it revealed a yet completer unity to be. And surely it had one great lesson, writ so large that all but the blind must have seen, and that is this: that science is the true bond of the nations, owing no allegiance save alone to truth, for which all the world may work in one spirit and by methods which are universal.

THE PRESENT PROBLEMS OF INORGANIC CHEMISTRY.*

By Sir WILLIAM RAMSAY, K.C.B., F.R.S.

TO discuss the 'present problems of inorganic chemistry' is by no means an easy task. The expression might be taken to mean an account of what is being actually done at present by those engaged in inorganic research; or it might be taken to relate to what needs doing—to the direction in which research is required. To summarize what is being done in an intelligible manner in the time at my disposal would be an almost impossible task; hence I will choose the latter interpretation of the title of my address. Now, a considerable experience in attempting to unveil the secrets of nature has convinced me that a deliberate effort to discover some new law or fact seldom succeeds. The investigator generally begins unmethodically, by random and chance experiments; or perhaps he is guided by some indication which has struck his attention during some previous research; and he is often the plaything of circumstances in his choice. Experience leads him to choose problems which most readily admit of solution, or which appear likely to lead to the most interesting results. If I may be excused the egotism of referring to my own work, I may illustrate what I mean by relating the following curious coincidence: After Lord Rayleigh had announced his discovery that 'atmospheric nitrogen' was denser than 'chemical nitrogen,' I referred to Cavendish's celebrated paper on the combination of the nitrogen and the oxygen of the air by means of electric sparks. Fortified by what I read, and by the knowledge gained during the performance of lecture-experiments that red-hot magnesium is a good and fairly rapid absorbent of nitrogen, it was not long before a considerable quantity of nearly pure argon had been separated from atmospheric nitrogen. Now it happened that I possess two copies of Cavendish's works; and some months afterwards I consulted the other copy and found penciled on the margin the words 'look into this.' I remembered the circumstance which led to the annotation. About ten years before, one of my students had investigated the direct combination of nitrogen and hydrogen, and I had read Cavendish's memoir on that occasion. I mention this fact to show that for some reason which I forget, a line of work was not followed up, which would have been attended by most interesting results; one does not always follow the clue

* An address at the International Congress of Arts and Science, St. Louis, September, 1904.

which yields results of the greatest interest. I regard it therefore as an impossible task to indicate the lines on which research should be carried out. All that I can do is to call attention to certain problems awaiting solution; but their relative importance must necessarily be a matter of personal bias, and others might with perhaps greater right suggest wholly different problems.

The fundamental task of inorganic chemistry is still connected with the classification of elements and compounds. The investigation of the classification of carbon compounds forms the field of organic chemistry; while general or physical chemistry deals with the laws of reaction, and the influence of various forms of energy in furthering or hindering chemical change. And classification centers at present in the periodic arrangement of the elements, according to the order of their atomic weights. Whatever changes in our views may be concealed in the lap of the future, this great generalization, due to Newlands, Lothar Meyer and Mendelejev, will always retain a place, perhaps the prominent place, in chemical science.

Now it is certain that no attempt to reduce the irregular regularity of the atomic weights to a mathematical expression has succeeded; and it is, in my opinion, very unlikely that any such expression, of not insuperable complexity, and having a basis of physical meaning, will ever be found. I have already, in an address to the German Association at Cassel, given an outline of the grand problem which awaits solution. It can be shortly stated then: While the factors of kinetic and of gravitational energy, velocity and momentum, on the one hand, and force and distance, on the other, are simply related to each other, the capacity factors of other forms of energy—surface, in the case of surface-energy; volume, in the case of volume-energy; entropy for heat; electric capacity when electric charges are being conveyed by means of ions; atomic weight, when chemical energy is being gained or lost—all these are simply connected with the fundamental chemical capacity, atomic weight or mass. The periodic arrangement is an attempt to bring the two sets of capacity factors into a simple relation to each other; and while the attempt is in so far a success, inasmuch as it is evident that some law is indicated, the divergences are such as to show that finality has not been attained. The central problem in inorganic chemistry is to answer the question—why this incomplete concordance? Having stated the general question, it may conduce to clearness if some details are given.

1. The variation of molecular surface energy with temperature is such that the surface-energy, for equal numbers of molecules distributed over a surface, is equal for equal intervals of temperature below the temperature at which surface-energy is zero—that is, the critical point. This gives a means of determining the molecular weights of liquids, and we assume that the molecular weight of a compound is accurately the sum of the atomic weights of the constituent elements.

2. The volume-energy of gases is equal at equal temperature from that at which volume-energy is zero—*i. e.*, absolute zero. And it follows that those volumes of gases which possess equal volume-energy contain equal numbers of molecules—again, a close connection with atomic weights.

3. The specific heats of elements are approximately inversely proportional to their atomic weights; and of compounds to the quotient of their molecular weights divided by the number of atoms in the molecule. Specific heat and entropy are closely related; hence one of the factors of thermal energy is proportional (nearly) to the reciprocal of the atomic weights.

4. The ion carries in its migration through a solution one or more electrons. Now, the ion is an atom carrying one or more charges—one for each equivalent. Here we have the capacity for electric charge proportional to the equivalent.

5. The factors of chemical energy are atomic weight and chemical potential; and as the former is identical numerically, or after multiplication by a simple factor with equivalent, electric potential is proportional to chemical potential.

We see therefore that surface, volume, thermal, electrical, and, no doubt, other forms of energy have as capacity factors magnitudes, either identical with, or closely related to, units of chemical capacity; while kinetic and linear energy are not so related, except through the periodic arrangement of the elements.

It appears therefore to be a fundamental problem for the chemist to ascertain, first, accurate atomic weights, and, second, to investigate some anomalies which still present difficulties. In America, you have excellent workers in the former branch. Mallet, Morley, Richards and many others have devoted their time and skill to perhaps the best work of this kind which has been done; and F. W. Clarke has collated all results and afforded incalculable help to all who work at or are interested in the subject. Valuable criticisms, too, have been made by Hinrichs; but it must be confessed that in spite of these, which are perhaps the best determinations which have been made, the problem becomes more, and not less formidable.

There are lines of work, however, which suggest themselves as possibly likely to throw light on the question. First, there is a striking anomaly in the atomic weight of nitrogen, determined by analysis and determined by density. Stas obtained the number 14.04 (O = 16), and Richards has recently confirmed his results; while Rayleigh and Leduc consistently obtained densities which, even when corrected so as to equalize the numbers of molecules in equal volumes, give the lower figure 14.002. The difference is 1 in 350; far beyond any possible experimental error. Recently, an attempt to combine the two methods has led to a mean number; but that result can hardly be taken as final. What is the reason of the discrepancy? Its discovery will surely ad-

vance knowledge materially. I would suggest the preparation of pure compounds of nitrogen, such as salts of hydrazine, methylanine, etc., and their careful analysis; and also the accurate determination of the density and analysis of such gaseous compounds of nitrogen as nitric oxide and peroxide. I have just heard from my former student, W. R. W. Gray, that he has recovered Stas's number by combining 2NO with O_2; while the density of NO leads to the lower value for the atomic weight of nitrogen.

The question of the atomic weight of tellurium appears to be settled, at least so far as its position with regard to the generally accepted atomic weight of iodine is concerned; recent determinations give the figures 127.5 (Gutbier), 127.6 (Pellini), and 127.9 (Köthner). But is that of iodine as accurately known? It would appear advisable to revise the determination of Stas, preparing the iodine preferably from an organic compound, such as iodoform, which can be produced in a high state of purity. The heteromorphism of selenates and tellurates, too, has recently been demonstrated; and it may be questioned whether these elements should both belong to the same group.

The rare earths still remain a puzzle. Their number is increasing yearly, and their claim to individuality admits of less and less dispute. What is to be done with them? Are they to be grouped by themselves as Brauner and Steele propose? If so, how is their connection with the other elements to be explained? Recent experiments in my laboratory have convinced me that in the case of thorium, at least, ordinary tests of purity such as fine crystals, constant subliming point, etc., do not always indicate homogeneity; or else that we are sadly in want of some analytical method of sufficient accuracy. The change of thorium into thorium X is perhaps hardly an explanation of the divergencies; yet it must be considered; but of this, more anon.

To turn next to another problem closely related to the orderly arrangement of the elements—that of valency—but little progress can be chronicled. The suggestions which have been made are speculative, rather than based on experiment. The existence of many peroxidized substances, such as percarbonates, perborates, persulphates and of crystalline compounds of salts with hydrogen peroxide, makes it difficult to draw any indisputable conclusions as regards valency from a consideration of oxygen compounds. Moissan's brilliant work on fluorides, however, has shown that SF_6 is capable of stable existence, and this forms a strong argument in support of the hexad character of sulphur. The tetravalency of oxygen, under befitting conditions, too, is being acknowledged, and this may be reconciled with the existence of water of crystallization, as well as of the per-salts already mentioned. The adherence of ammonia to many chlorides, nitrates, etc., points to the connecting link being ascribable to the pentavalency of nitrogen; and it might be worth while investigating similar compounds

with phosphoretted and arsenicoretted hydrogen, especially at low temperatures.

The progress of chemical discovery, indeed, is closely connected with the invention of new methods of research, or the submitting of matter to new conditions. While Moissan led the way by elaborating the electric furnace, and thus obtained a potent agent in temperatures formerly unattainable, Spring has tried the effect of enormous pressure, and has recently found chemical action between cuprous oxide and sulphur at ordinary temperature, provided the pressure be raised to 8,000 atmospheres. Increase of pressure appears to lower the temperature of reaction. It has been known for long that explosions will not propagate in rarefied gases, and that they become more violent when the reacting gases are compressed: but we are met with difficulties, such as the non-combination of hydrogen and nitrogen, even at high temperature and great pressure; yet it is possible to measure the electromotive force (0.59 volt) in a couple consisting of gaseous nitrogen and gaseous hydrogen, the electrolyte being a solution of ammonium nitrate saturated with ammonia. Chemical action between dissolved hydrogen and nitrogen undoubtedly occurs; but it is not continuous. Again we may ask, Why? The heat evolution should be great; the gain of entropy should also be high were direct combination to occur. Why does it not occur to any measurable extent? Is it because for the initial stages of any chemical reaction, the reacting molecules must be already dissociated, and those of nitrogen are not? Is that in any way connected with the abnormally low density of gaseous nitrogen? Or is it that, in order that combination shall occur, the atoms must fit each other; and that in order that nitrogen and hydrogen atoms may fit, they must be greatly distorted? But these are speculative questions, and it is not obvious how experiments can be devised to answer them.

Many compounds are stable at low temperatures which dissociate when temperature is raised. Experiments are being made, now that liquid air is to be purchased or cheaply made, on the combinations of substances which are indifferent to each other at ordinary temperatures. Yet the research must be a restricted one, for most substances are solid at $-185°$, and refuse to act on each other. It is probable, however, that at low temperatures compounds could be formed in which one of the elements would possess a greater valency than that usually ascribed to it; and also that double compounds of greater complexity would prove stable. Valency, indeed, appears to be in many cases a function of temperature; exothermic compounds, as is well known, are less stable, the higher the temperature. The sudden cooling of compounds produced at a high temperature may possibly result in forms being preserved which are unstable at ordinary temperatures. Experiments have been made in the hope of obtaining compounds of argon and helium by exposing various elements to the influence of sparks from

a powerful induction coil, keeping the walls of the containing-vessel at the temperature of liquid air, in the hope that any endothermic compound which might be formed would be rapidly cooled, and would survive the interval of temperature at which decomposition would take place naturally. But these experiments have so far yielded only negative results. There is some indication, however, that such compounds are stable at 1,500°. It might be hoped that a study of the behavior of the non-valent elements would have led to some conception of the nature of valency; but so far, no results bearing on the question have transpired. The condition of helium in the minerals from which it is obtainable by heat is not explained; and experiments in this direction have not furnished any positive information. It is always doubtful whether it is advisable to publish the results of negative experiments; for it is always possible that some more skilled or more fortunate investigator may succeed, where one has failed. But it may be chronicled that attempts to cause combination between the inactive gases and lithium, potassium, rubidium, and cæsium have yielded no positive results; nor do they appear to react with fluorine. Yet conditions of experiment play a leading part in causing combination, as has been well shown by Moissan with the hydrides of the alkali-metals, and by Guntz, with those of the metals of the alkaline earths. The proof that sodium hydride possesses the formula NaH, instead of the formerly accepted one, removes one difficulty in the problem of valency; and SrH_2 falls into its natural position among hydrides.

A fertile field of inorganic research lies in the investigation of structure. While the structure of organic compounds has been elucidated almost completely, that of inorganic compounds is practically undeveloped. Yet efforts have been made in this direction which appear to point a way. The nature of the silicates has been the subject of research for many years by F. W. Clarke; and the way has been opened. Much may be done by treating silicates with appropriate solvents, acid or alkaline, which differentiate between uncombined and combined silica, and which in some cases, by replacement of one metal by another, gives a clue to constitution. The complexity of the molecules of inorganic compounds, which are usually solid, forms another bar to investigation. It is clear that sulphuric acid, to choose a common instance, possesses a very complicated molecule; and the fused nitrates of sodium and potassium are not correctly represented by the simple formulæ $NaNO_3$ and KNO_3. Any theory of the structure of their derivatives must take such facts into consideration; but we appear to be getting nearer the elucidation of the molecular weights of solids. Again, the complexity of solutions of the most common salts is maintained by many investigators; for example, a solution of cobalt chloride, while it undoubtedly contains among other constituents simple molecules of $CoCl_2$, also consists of ions of a complex character, such as

$(CoCl_4)''$. And what holds for cobalt chloride also undoubtedly holds for many similar compounds.

In determining the constitution of the compounds of carbon, stereochemistry has played a great part. The ordinary structural formulæ are now universally acknowledged to be only pictorial, if, indeed, that word is legitimate; perhaps it would be better to say that they are distorted attempts at pictures, the drawing of which is entirely free from all rules of perspective. But these formulæ may in almost every case be made nearly true pictures of the configuration of the molecules. The benzine formula, to choose an instance which is by no means the simplest, has been shown by Collie to be imitated by a model which represents in an unstrained manner the behavior of that body on treatment with reagents. But in the domain of inorganic chemistry, little progress has been made. Some ingenious ideas of the geologist Sollas on this problem have hardly received the attention which they deserve; perhaps they may have been regarded as too speculative. On the other hand, Le Bel's and Pope's proof of the stereo-isomerism of certain compounds of nitrogen; Pope's demonstration of the tetrahedral structure of the alkyl derivatives of tin; and Smiles's syntheses of stereo-isomeric sulphur compounds give us the hope that further investigation will lead to the classification of many other elements from this point of view. Indeed, the field is almost virgin soil; but it is well worth while cultivating. There is no doubt that the investigation of other organo-metallic compounds will result in the discovery of stereo-isomerides; yet the methods of investigation capable of separating such constituents have in most cases still to be discovered.

The number of chemical isomerides among inorganic compounds is a restricted one. Werner has done much to elucidate this subject in the case of complex ammonia derivatives of metals and their salts; but there appears to be little doubt that if looked for, the same or similar phenomena would be discoverable in compounds with much simpler formulæ. The two forms of So_3, sulphuric anhydride, are an instance in point. No doubt formation under different conditions of temperature and pressure might result in the greater stability of some forms which under our ordinary conditions are changeable and unstable. The fact that under higher pressures than are generally at our disposal different forms of ice have been proved to exist, and the application of the phase-rule to such cases, will greatly enlarge our knowledge of molecular isomerism.

The phenomena of catalysis have been extensively studied of recent years, and have obviously an important bearing on such problems. A catalytic agent is one which accelerates or retards the velocity of reaction. Without inquiring into the mechanism of catalysis, its existence may be made to influence the rate of chemical change, and to render stable bodies which under ordinary conditions are unstable. For if it is possible to accelerate a chemical change in such a way that the

usually slow and possibly unrecognizable rate of isomeric change may be made apparent and measurable, a substance the existence of which could not be recognized under ordinary circumstances, owing to its infinitesimal amount, may be induced to exist in weighable quantity, if the velocity of its formation from an isomeride can be greatly accelerated by the presence of an appropriate catalytic agent. I am not aware that attempts have been made in this direction. The discovery of catalytic agents is, as a rule, the result of accident. I do not think that any guide exists which would enable us to predict that any particular substance would cause an acceleration or a retardation of any particular reaction. But catalytic agents are generally those which themselves, by their power of combining with or parting with oxygen, or some other element, cause the transfer of that element to other compounds to take place with increased or diminished velocity. It is possible, therefore, to cause ordinary reactions to take place in presence of a third body, choosing the third body with a view to its catalytic action, and to examine carefully the products of the main reaction as regards their nature and their quantity. Attempts have been made in this direction with marked success; the rate of change of hydrogen dioxide, for example, has been fairly well studied. But what has been done for that compound may be extended indefinitely to others, and doubtless with analogous results. Indications of the existence of as yet undiscovered compounds may be derived from a study of physical, and particularly of electrical, changes. There appears to be sufficient evidence of an oxide of hydrogen containing more oxygen than hydrogen dioxide, from a study of the electromotive force of a cell containing hydrogen dioxide; yet the higher oxide still awaits discovery.

The interpretation of chemical change in the light of the ionic theory may now be taken as an integral part of inorganic chemistry. The ordinary reactions of qualitative and quantitative analysis are now almost universally ascribed to the ions, not to the molecules. And the study of the properties of most ions falls into the province of the inorganic chemist. To take a familiar example: The precipitation of hydroxides by means of ammonia-solution has long led to the hypothesis that the solution contained ammonium hydroxide; and, indeed, the teaching of the text-books and the labels on the bottles supported this view. But we know now that a solution of ammonia in water is a complex mixture of liquid ammonia and liquid water; of ammonium hydroxide, NH_4OH; and of ions of ammonium $(NH_4)'$, and hydroxyl $(OH)'$. Its reactions, therefore, are those of such a complex mixture. If brought into contact with a solution of some substance which will withdraw the hydroxyl ions, converting them into water, or into some non-ionized substance, they are replaced at the expense of the molecules of non-ionized ammonium hydroxide; and these, when diminished in amount, draw on the store of molecules of ammonia and water, which combine, so as to maintain equilibrium. Now the investigation of such

changes must belong to the domain of inorganic chemistry. It is true that the methods of investigation are borrowed from the physical chemist; but the products lie in the province of the inorganic chemist. Indeed, the different departments of chemistry are so interlaced that it is impossible to pursue investigations in any one branch without borrowing methods from the others; and the inorganic chemist must be familiar with all chemistry, if he is to make notable progress in his own branch of the subject. And if the substances and processes investigated by the inorganic chemist are destined to become commercially important, it is impossible to place the manufacture on a sound commercial basis without ample knowledge of physical methods, and their application to the most economical methods of accelerating certain reactions and retarding others, so as to obtain the largest yield of the required product at the smallest cost of time, labor and money.

I have endeavored to sketch some of the aspects of inorganic chemistry with a view to suggesting problems for solution, or at least the directions in which such problems are to be sought. But the developments of recent years have been so astonishing and so unexpected, that I should fail in my duty were I not to allude to the phenomena of radioactivity, and their bearing on the subject of my address. It is difficult to gauge the relative importance of investigations in this field; but I may be pardoned if I give a short account of what has already been done, and point out lines of investigation which appear to me likely to yield useful results.

The wonderful discovery of radium by Madame Curie, the preparation of practically pure compounds of it, and the determination of its atomic weight are familiar to all of you. Her discovery of polonium, and Debierne's of actinium have also attracted much attention. The recognition of the radioactivity of uranium by Becquerel, which gave the first impulse to these discoveries, and of that of thorium by Schmidt, is also well known.

These substances, however, presented at first more interest for the physicist than the chemist, on account of the extraordinary power which they all possess of emitting 'rays.' At first, these rays were supposed to constitute ethereal vibrations; but all the phenomena were not explicable on that supposition. Schmidt first, and Rutherford and Soddy later, found that certain so-called 'rays' really consist of gases; and that while thorium emits one kind, radium emits another; and no doubt Debierne's actinium emits a third. The name 'emanations' was applied by Rutherford to such radioactive bodies; he and Soddy found that those of radium and thorium could be condensed and frozen by exposure to the temperature of liquid air, and that they were not destroyed or altered in any way by treatment with agents which are able to separate all known gases from those of the argon group, namely, red-hot magnesium-lime, and it was later found that sparking with oxygen in presence of caustic potash did not affect the

gaseous emanation from radium. The conclusion therefore followed that in all probability these bodies are gases of the argon group, the atomic weight of which, and consequently the density, is very high; indeed, several observers, by means of experiments on the rate of diffusion of the gas from radium, believe it to have a density of approximately 100, referred to the hydrogen standard. This conclusion has been confirmed by the mapping of the spectrum of the radium emanation, which is similar in general character to the spectra of the inactive gases, consisting of a number of well-defined, clearly cut brilliant lines, standing out from a black background. The volume of the gas produced spontaneously from a given weight of radium bromide in a given time has been measured; and it was incidentally shown that this gas obeys Boyle's law of pressures. The amount of gas thus collected and measured, however, was very minute; the total quantity was about the forty-thousandth of a cubic centimeter.

Having noticed that those minerals which consist of compounds of uranium and thorium contain helium, Rutherford and Soddy made the suggestion that it might not be impossible that helium is the product of the spontaneous change of the emanation; and Soddy and I were able to show that this is actually the case. For, first, when a quantity of a radium salt which has been prepared for some time is dissolved in water, the occluded helium is expelled, and can be recognized by means of its spectrum; further, the fresh emanation shows no helium spectrum, but after a few days the spectrum of helium begins to appear, proving that a spontaneous change is in progress; and last, as the emanation disappears its volume decreases to zero; and on heating the capillary glass tube which contained it, helium is driven out from the glass walls, into which its molecules had been imbedded in volume equal to three and a half times that of the emanation. The α-rays, as foreshadowed by Rutherford and Soddy, consist of helium particles.

All these facts substantiate the theory, devised by Rutherford and Soddy, that the radium atom is capable of disintegration, one of the products being a gas, which itself undergoes further disintegration, forming helium as one of its products. Up till now, the sheet anchor of the chemists has been the atom. But the atom itself appears to be complex, and to be capable of decomposition. It is true that only in the case of a very few elements, and these of high atomic weight, has this been proved. But even radium, the element which has by far the most rapid rate of disintegration, has a comparatively long life; the period of half-change of any given mass of radium is approximately 1,100 years. The rate of change of the other elements is incomparably slower. This change, too, at least in the case of radium, and its emanation, and presumably also in the case of other elements, is attended with an enormous loss of energy. It is easy to calculate from heat measurements (and independent and concordant measurements have been made) that one pound of emanation is capable of parting with

as much energy as several hundred tons of nitroglycerine. The order of the quantity of energy evolved during the disintegration of the atom is as astonishing as the nature of the change. But the nature of the change is parallel to what would take place if an extremely complicated hydrocarbon were to disintegrate; its disruption into simpler paraffins and olefines would also be attended with loss of energy. We may therefore take it, I think, that the disintegration hypothesis of Rutherford and Soddy is the only one which will meet the case.

If radium is continually disappearing, and would totally disappear in a very few thousand years, it follows that it must be reproduced from other substances, at an equal rate. The most evident conjecture, that it is formed from uranium, has not been substantiated. Soddy has shown that salts of uranium, freed from radium, and left for a year, do not contain one ten-thousandth part of the radium that one would expect to be formed in the time. It is evident therefore that radium must owe its existence to the presence of some other substances, but what they are is still unascertained.

During the investigation of Rutherford and Soddy of the thorium emanation, a most interesting fact was observed, namely, that precipitation of the thorium as hydroxide by ammonia left unprecipitated a substance, which they termed thorium-X, and which was itself highly radio-active. Its radio-active life, however, was a short one; and as it decayed, it was reproduced from its parent thorium at an equal rate. Here is a case analogous to what was sought for with radium and uranium; but evidently uranium is not the only parent of radium; the operation is not one of parthenogenesis. Similar facts have been elicited for uranium by Crookes.

The α-rays, caused by the disintegration of radium and of its emanation, are accompanied by rays of quite a different character; they are the β-rays, identical with electrons, the mass of which has been measured by J. J. Thomson and others. These particles are projected with enormous velocity, and are capable of penetrating glass and metal screens. The power of penetration appears to be proportional to the amount of matter in the screen, estimated by its density. These electrons are not matter; but, as I shall relate, they are capable of causing profound changes in matter.

For the past year, a solution of radium bromide has been kept in three glass bulbs each connected to a Topler pump by means of capillary tubing. To ensure these bulbs against accident, each was surrounded by a small beaker; it happened that one of these beakers consisted mainly of potash glass; the other two were of soda glass. The potash-glass beaker became brown, while the two soda-glass beakers became purple. I think there is every probability that the colors are due to liberation of the metals potassium and sodium in the glass. They are contained in that very viscous liquid, glass, in the colorless ionic state; but these ions are discharged by the β-rays, or negative

electrons, and each metal imparts its own peculiar color to the glass, as has been shown by Maxwell Garnett. This phenomenon, however interesting, is not the one to which I desire to draw special attention. It must be remembered that the beakers have been exposed only to β-rays; α-rays have never been in contact with them; they have never been bombarded by what is usually called matter, except by the molecules of the surrounding air. Now these colored beakers are radioactive, and *the radioactive film dissolves in water.* After careful washing, the glass was no longer radioactive. The solution contains an emanation, for on bubbling air through it, and cooling the issuing air with liquid air, part of the radioactive matter was retained in the cooled tube. This substance can be carried into an electroscope by a current of air, after the liquid air has been withdrawn, and as long as the air-current passes, the electroscope is discharged; the period of decay of this emanation, however, is very rapid, and on ceasing the current of air, the leaves of the electroscope cease to be discharged. In having such a short period of existence, this emanation resembles the one from actinium.

Owing to the recess, only a commencement has been made with the investigation of the residue left on evaporation of the aqueous solution. On evaporation, the residue is strongly active. Some mercurous nitrate was then added to the dissolved residue, and it was treated with hydrochloric acid in excess, to precipitate mercurous chloride. The greater part of the active matter was thrown down with the mercurous chloride, hence it appears to form an insoluble chloride. The mercurous chloride retained its activity unchanged in amount for ten days. The filtrate from the mercurous chloride, on evaporation, turned out to be active; and on precipitating mercuric sulphide in it, the sulphide precipitate was also active; but its activity decayed in one day. The filtrate from the mercuric sulphide gave inactive precipitatates with ferric salts and ammonia, with zinc salts and ammonium sulphide, with calcium salts and ammonium carbonate; and on final evaporation, the residue was not radioactive. Hence the active matter forms an insoluble chloride and sulphide. The precipitated mercurous chloride and mercuric sulphide were dissolved in *aqua regia,* and the solution was evaporated. The residue was dissolved in water, and left the dish inactive. But the solution gave an insoluble sulphate, when barium chloride and sulphuric acid were added to it, hence the radioactive element forms an insoluble sulphate, as well as an insoluble chloride and sulphide.

This is a sample of the experiments which have been made. It may be remarked that the above results were obtained from a mixture of the potash and soda glass; somewhat different results were obtained from the potash glass alone. These changes appear to be due to the conversion of one or more of the constituents of the glass into other

bodies. Needless to say, neither of the samples of glass contained lead.

I have mentioned these experiments in detail, because I think they suggest wholly new lines of investigation. It would appear that if energy can be poured into a definite chemical matter, such as glass, it undergoes some change, and gives rise to bodies capable of being tested for; I imagine that radio-active forms of matter are produced, either identical with or allied to, those at present known. And just as radium and other radio-active elements suffer degradation spontaneously, evolving energy, so I venture to think that if energy be concentrated in the molecules of ordinary forms of matter, a sort of polymerization is the result, and radio-active elements, probably elements with high atomic weight, and themselves unstable, are formed. Of course further research may greatly modify these views; but some guide is necessary, and Mr. Ternent Cook, who has helped me in these experiments, and I suggest this hypothesis (in the words of Dr. Johnstone Stoney, a hypothesis is 'a supposition which we hope may be useful') to serve as a guide for future endeavor.

In the light of such facts, speculation on the periodic arrangement of the elements is surely premature. It is open to any one to make suggestions; they are self-evident. Most of you will agree with the saying 'it is easy to prophesy after the event.' I prefer to wait until prophecy becomes easy.

I must ask your indulgence for having merely selected a few out of the many possible views as regards the Problems of Inorganic Chemistry. I can only plead in excuse that my task is not an easy one; and I venture to express the hope that some light has been thrown on the shady paths which penetrate that dark region which we term the future.

THE LIGHT OF THE STARS.*

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IF an intelligent observer should see the stars for the first time, two of their properties would impress him as subjects for careful study; first, their relative positions, and secondly, their relative brightness. From the first of these has arisen the astronomy of position, or astrometry. This is sometimes called the old astronomy, since until within the last twenty years the astronomers of the world, with few exceptions, devoted their attention almost entirely to it. To the measure of the light should be added the study of the color of the stars (still in its infancy), and the study of their composition by means of the spectroscope. In this way a young giant has been reared, which has almost dwarfed its older brothers. The science of astrophysics, or the new astronomy, has thus been developed, which during the last few years has rejuvenated the science and given to it, by its brilliant discoveries, a public interest which could not otherwise have been awakened. The application to stellar astronomy, of the daguerreotype in 1850, of the photograph in 1857, and of the dry plate in 1882, has opened new fields in almost every department of this science. In some, as in stellar spectroscopy, it has almost completely replaced visual observations.

One department of the new astronomy, the relative brightness of the stars, is as old as, or older than, the old astronomy. An astronomer even now might do useful work in this department without any instruments whatever. Hipparchus is known to have made a catalogue of the stars about 150 B. C. Ptolemy, in 138 A. D., issued that great work, the Almagest, which for fourteen hundred years constituted the principal and almost the sole authority in astronomy. It contained a catalogue of 1,028 stars, perhaps based on that of Hipparchus. Ptolemy used a scale of stellar magnitudes which has continued in use to the present day. He called the brightest stars in the sky, the first magnitude, the faintest visible to the naked eye, the sixth. More strictly, he used the first six letters of the Greek alphabet for this purpose. But he went a step further, and subdivided these classes. If a star seemed bright for its class, he added the letter μ (mu), standing for $\mu\epsilon\iota\zeta\omega\nu$ (meizon), large or bright; if the star was faint, he added ϵ (epsilon), standing for $\epsilon\lambda\alpha\sigma\sigma\omega\nu$ (elasson), small or faint. These

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estimates were presumably carefully made, and if we had them now, they would be of the greatest value in determining the secular changes, if any, in the light of the stars. The earliest copy we have of the Almagest is No. 2,389 of the collection in the Bibliothèque Nationale of Paris. It is a beautiful manuscript written in the uncial characters of the ninth century. A few years ago it could be seen by any one in one of the show cases of the library. There are many later manuscripts and printed editions which have been compared by various students. The errors in these various copies are so numerous that there is an uncertainty in the position, magnitude, or identification of about two thirds of the stars. A most important revision was made by the Persian astronomer, Abd-al-rahman al-sufi, who reobserved Ptolemy's stars, A. D. 964, and noted the cases in which he found a difference. The careful study and translation of this work from Arabic into French by Schjellerup has rendered it readily accessible to modern readers.

No important addition to our knowledge of the light of the stars was made until the time of Sir William Herschel, the greatest of modern observers. He found that when two stars were nearly equal, the difference could be estimated very accurately. He designated these intervals by points of punctuation, a period denoting equality, a comma a very small interval, and a dash a larger interval. In 1796 to 1799 he published, in the *Philosophical Transactions,* four catalogues, covering two thirds of the portion of the sky visible in England. Nearly a century later, it was my great good fortune, when visiting his grandson, to discover in the family library the two catalogues required to complete this work, and which had not been known to exist. These two catalogues are still unpublished. Meanwhile, little or no use had been made of the four published catalogues which, while comparing one star with another, furnished no means of reducing all to one system of magnitudes. The Harvard measures permitted me to do this for all six catalogues, and thus enabled me to publish magnitudes for 2,785 stars observed a century ago, with an accuracy nearly comparable with the best work of the present time. For nearly half a century no great advance was made, and no astronomer was wise enough to see how valuable a work he could do by merely repeating the observations of Herschel. Had this work been extended to the southern stars, and repeated every ten years, our knowledge of the constancy of the light of the stars would have been greatly increased. In 1844, Argelander proposed, in studying variable stars, to estimate small intervals modifying the method of Herschel by using numbers instead of points of punctuation, and thus developed the method known by his name. This is now the best method of determining the light of the stars, when only the naked eye or a telescope is available, and much valuable work might be done by applying it to the fainter stars, and especially to clusters.

Meanwhile photometric measures of the stars, according to various methods, had been undertaken. In 1856, Pogson showed that the scale of magnitudes of Ptolemy, which is still in use, could be nearly represented by assuming the unit to be the constant ratio, 2.512, whose logarithm is 0.4. This has been generally adopted as the basis of the standard photometric scale. The photometer devised by Zöllner has been more widely used than any other. In this instrument an artificial star is reduced any desired amount, by polarized light, until it appears to equal the real star, both being seen side by side in the telescope. Work with this instrument has attained its greatest perfection at the Potsdam Observatory, where measures of the light of the northern stars, whose magnitude is 7.5 and brighter, have been in progress since 1886. The resulting magnitudes have been published for 12,046 stars, included in declination between — 2° and — 60°. The accidental errors are extremely small, but as the results of different catalogues differ systematically from one another, we can not be sure which is right and what is the real accuracy attained in each case. In 1885 the Uranometria Oxoniensis was published. It gives the magnitudes of 2,784 northern stars north of declination — 10°. This work is a remarkable one, especially as its author, Professor Pritchard, began his astronomical career at the age of sixty-three. The method he employed was reducing the light of the stars by means of a wedge of shade glass until they became invisible, and then determining the brightness from the position of the wedge. A careful and laborious investigation, extending over many years has been carried on by Mr. H. M. Parkhurst, using a modification of this method.

For several years before the Oxford and Potsdam measures described above were undertaken, photometric observations were in progress at Harvard. In 1877 a large number of comparisons of adjacent stars were made with a polarizing photometer. Two images of each star were formed with a double image prism, and the relative brightness was varied by turning a Nicol prism until the ordinary image of one star appeared equal to the extraordinary image of the other. Several important sources of error were detected, which once known were easily eliminated. A bright star will greatly affect the apparent brightness of an adjacent faint one, the error often exceeding a magnitude. Systematic errors amounting to several tenths of a magnitude depend upon the relative positions of the images compared. They are perhaps due to the varying sensitiveness of the different parts of the retina. This photometer has many important advantages. However bad the images may be, they are always exactly alike, and may, therefore, be compared with accuracy. Both stars are affected equally by passing clouds, so that this photometer may be used whenever the stars are visible and at times when other photometric work is impossible. The diminution in light also follows a simple geometrical law, and is readily computed with great accuracy. There is no unknown constant to be

determined, as in the Pritchard, and nearly all other photometers. The principal objections to this instrument are, first, that stars can not be compared unless they are near together, and, secondly, that faint stars can not be measured, since one half of the light is lost by polarization. The principal uses so far made of this form of photometer are in comparing the components of double stars, and in a long series of observations of the eclipses of Jupiter's satellites, which now extend over a quarter of a century and includes 768 eclipses. Instead of observing the time of disappearance, a series of measurements is made, which gives a light curve for each eclipse. Much important work might yet be done with this form of photometer, in measuring the components of doubles and of clusters, and in determining the light curves of variables which have a moderately bright star near them.

An important improvement was made in this form of photometer in 1892 by which stars as much as half a degree apart could be compared. The cones of light of two such stars are brought together by achromatic prisms, so that they can be compared as in the preceding instrument. As there is no part of the sky in which a suitable comparison star can not be found within this distance, any star may be measured with this instrument. In the hands of Professor Wendell this photometer has given results of remarkable precision. The average deviation of the result of a set of sixteen settings is about three hundredths of a magnitude. Light curves of variables can therefore be determined with great precision, and suspected variables can be divided into those that are certainly variable, and those whose changes are probably less than a tenth of a magnitude.

Another change in this instrument produced the meridian photometer. Instead of using the two cones from one object glass, two object glasses were used, mirrors being placed in front of each. In this way stars however distant can be compared. In theory this instrument leaves but little to be desired. Almost every source of error that can be suggested can be eliminated by proper reversion. As constructed, the telescope is placed horizontally, pointing east or west. One mirror reflects a star near the pole into the field, the other, a star upon the meridian. A slight motion of the mirror permits stars to be observed for several minutes before or after culmination. The first meridian photometer had objectives of only two inches aperture. With this instrument 94,476 measures were made of 4,260 stars during the years 1879 to 1882. All stars were included of the sixth magnitude and brighter, and north of declination — 30°. The second instrument had objectives of four inches aperture, and permitted stars as faint as the tenth magnitude to be measured. With this instrument, during the years 1882 to 1888, 267,092 measures were made of 20,982 stars, including all the catalogue stars and all the stars of the ninth magnitude and brighter, in zones twenty minutes wide, and at intervals of

five degrees, from the north pole to declination — 20°. In 1889 the instrument was sent to South America, where 98,744 measures were made of 7,922 southern stars, extending the two preceding researches to the South Pole. On the return of the instrument to Cambridge 473,216 measures were made of 29,587 stars, including all those of the magnitude 7.5 and brighter north of declination — 30°. This work occupied the years 1891 to 1898. The instrument was again sent to Peru in 1899, and 50,816 measures were made of 5,332 stars, including all those of the seventh magnitude and brighter, south of declination — 30°. The latest research has been the measurement of a series of stars of about the fifth magnitude, one in each of a series of regions ten degrees square. Each of these stars is measured with the greatest care on ten nights. This work has been completed and published for stars north of declination — 30°, 59,428 measures having been made of 839 stars. In this count, numerous other stars have been included. Similar measures are now in progress of the southern stars, this being the third time the meridian photometer has been sent to South America. The total number of measurements exceeds a million, and the number of stars is about sixty thousand. About sixty stars can be identified with care, and each measured four times with this instrument in an hour. The probable error of a set of four settings is ± 0.08.

The principal objection to the instrument just described is the great loss of light. To measure very faint stars, another type of photometer has been devised. A twelve-inch telescope has been mounted horizontally, like the meridian photometer, and an artificial star reflected into the field. The light of this star is reduced by a wedge of shade glass until it appears equal to the star to be measured. Four hundred thousand measures have been made with this instrument during the last five years. The principal research has been the measurement of all the stars in the Bonn Durchmusterung which are contained in zones ten minutes wide and at intervals of five degrees, from the north pole to declination — 20°. Large numbers of stars of the tenth and eleventh magnitudes are thus furnished as standards of light. As the light of the object observed is unobstructed, any star however faint, if visible in the telescope, may be measured. Accordingly, many stars of the twelfth and thirteenth magnitude have been selected and measured, thus furnishing faint standards. Sequences of standard stars have been selected from coarse clusters, thus permitting estimates or measures of these bodies to be reduced to a uniform photometric scale. An investigation of great value has been carried out successfully at the Georgetown College Observatory by the Rev. J. G. Hagen, S.J. All the stars of the thirteenth magnitude and brighter have been catalogued and charted in a series of regions, each one degree square, surrounding variable stars of long period. Besides measuring the positions he has determined the relative brightness of these stars. A sequence has then been selected from each of these regions, and meas-

ured at Harvard with the twelve-inch meridian photometer, thus permitting all to be reduced to a uniform scale. As the photometer was first constructed, stars brighter than the seventh magnitude could not be measured, since they were brighter than the artificial star and could not be rendered equal to it. This difficulty was remedied by inserting a series of shades, the densest of which reduced the light by ten magnitudes. By this method, the range of the photometer may be increased indefinitely. Sirius and stars of the twelfth magnitude have been satisfactorily measured in succession. A further modification of the instrument permitted surfaces to be compared. The light of the sky at night and in the daytime, during twilight, at different distances from the moon, and different portions of the disc of the latter, have thus been compared. Measures extending over seventeen magnitudes, with an average deviation of about three hundredths of a magnitude, were obtained in this way. One light was thus compared with another six million times as bright as itself. A slight modification would permit the intrinsic brightness of the different portions of the sun's disc to be compared with that of the faintest nebulæ visible. By these instruments, the scale of photometric magnitudes has been carried as far as the thirteenth magnitude. To provide standards for fainter stars, a small appropriation was made by the Rumford Committee of the American Academy. Cooperation was secured among the directors of the Yerkes, Lick, McCormick, Halsted and Harvard Observatories. Similar photometers were constructed for all, in which an artificial star was reduced any desired amount by a photographic wedge. Telescopes of 40, 36, 26, 23 and 15 inches aperture, including the two largest refractors in the world, were thus used in the same way on the same research. The standards have all been selected, and nearly all of the measurements have been made. This furnishes a striking illustration of the advantages of cooperation, and combined organization. When these observations are reduced, we shall have standards of magnitude according to a uniform scale, for all stars from the brightest to the faintest visible in the largest telescopes at present in use. The sixty-inch reflector of the late A. A. Common has recently been secured by the Harvard Observatory. It is hoped that still fainter stars may be measured with this instrument.

We have as yet only considered the total light of a star, so far as it affects the eye. But this light consists of rays of many different wave lengths. In red stars, one color predominates, in blue, another. The true method is to compare the light of a given wave-length in different stars, and then to determine the relative intensity of the rays of different wave-lengths in different stars, or at least in stars whose spectra are of different types. This is the only true method, and fortunately spectrum photography permits it to be done. The Draper catalogue gives the class of spectrum of 10,351 stars, and the relative brightness of the light whose wave-length is 430, is determined for each. In

1891, measures were published of the relative light of rays of various wave-lengths, for a number of stars whose spectra were of the first, second and third types.

A much simpler, but less satisfactory method, is to measure the total light in a photographic image. As in the case of eye photometry, this method is open to the objection that rays of different colors are combined. Blue stars will appear relatively brighter, and red stars relatively fainter, in the photograph than to the eye. This, however, is an advantage rather than an objection, since it appears to furnish the best practical measure of the color of the stars. Relative photographic magnitudes can be obtained in a variety of ways, and the real difficulty is to reduce them to an absolute scale of magnitudes. But for this, photographic might supersede photometric magnitudes. In other respects, photography possesses all the advantages for this work that it has for other purposes, and many photometric problems are within the reach of photography, which seems hopeless by visual methods. In 1857, Professor George P. Bond, the father of stellar photography, showed that the relative light of the stars could be determined from the diameter of their photographic images. This is the method that has been generally adopted elsewhere in determining photographic magnitudes, although with results that are far from satisfactory. It is singular that although this method originated at Harvard, it is almost the only one not in use here, while a great variety of other methods have been applied to many thousands of stars, during the last eighteen years. Relative measures are obtained very satisfactorily by applying the Herschel-Argelander method to photographic images, and if these could be reduced to absolute magnitudes, it would leave but little to be desired. In the attempt to determine absolute magnitudes a variety of methods has been employed. The simplest is to form a scale by photographing a series of images, using different exposures. The image of any star may be compared directly with such a scale. To avoid the uncertain correction due to the times of exposure, different apertures may be used instead of different exposures. Another method is to attach a small prism to the objective. The image of every bright star is then accompanied by a second image a few minutes of arc distant from it, and fainter by a constant amount, as five magnitudes. Trails may be measured more accurately than circular images, and trails of stars near the pole have varying velocities, which may then be compared with one another by means of a scale. Again, images out of focus may be compared with great accuracy and rapidity by means of a photographic wedge. These comparisons promise to furnish excellent magnitudes, if they can only be reduced to the photometric scale. A catalogue giving the photographic magnitudes of 1,131 stars within two degrees of the equator, and determined from their trails, was published in 1889. Great care was taken to eliminate errors due to right ascension, so that standards in remote portions of

the sky are comparable. A similar work on polar stars at upper and lower culmination determined the photographic absorption of the atmosphere, which is nearly twice as great as the visual absorption. A catalogue of forty thousand stars of the tenth magnitude, one in each square degree, has been undertaken, and the measures are nearly complete for the portion of the sky extending from the equator to declination + 30°. These stars are compared, by means of a scale, with the prismatic companions of adjacent bright stars. Two measures have been made of images out of focus of 8,489 stars, including all of those north of declination — 20°, and brighter than the seventh magnitude. This work is being continued to the south pole. The most important completed catalogue of photographic magnitudes is the 'Cape Photographic Durchmusterung,' the monumental work of Gill and Kapteyn. 454,875 stars south of declination — 19° are included in this work. Unfortunately, the difficulty mentioned above, of reducing the magnitudes to an absolute system, has not been wholly overcome, but the work is published in a form which will permit this to be done later, if a method of reduction can be discovered. The extension of this great work to the north pole is one of the greatest needs of astronomy at the present time.

The map and catalogue of the Astrophotographic Congress, the most extensive research ever undertaken by astronomers, will not be discussed here, as it will doubtless be described by others better able than I, to explain its merits. If completed, and if the difficulty of reducing the measures of brightness to a standard scale can be overcome, it will furnish the photographic magnitudes, as well as the positions, of two million stars. Time does not permit the consideration here of certain other investigations of photographic magnitudes, such as those made at Groningen. They generally relate to a comparatively small number of stars. The suggestion that the intensity of a photographic star image be measured by the amount of light it cuts off from a thermo-pile, deserves careful study. It should give a great increase in precision, and would eliminate that tool of many defects, the human eye. No use seems to have been made so far of this method.

The next question to be considered is, what use should be made of these various measures of the light of the stars? The most obvious application of them is to variable stars. While the greater portion of the stars undergo no changes in light that are perceptible, several hundred have been found whose light changes. A natural classification seems to be that proposed by the writer in 1880. A few stars appear suddenly, and are called new stars, or novæ. They form class I. Class II. consists of stars which vary by a large amount during periods of several months. They are known as variable stars of long period. Class III. contains stars whose variations are small and irregular. Class IV. contains the variable stars of short period, and Class V. the Algol variables, which are usually of full brightness but at regular

intervals grow faint, owing to the interposition of a dark companion. Twenty years ago, when photography was first applied to the discovery of variable stars, only about two hundred and fifty of these objects were known. Since then, three remarkable discoveries have been made, by means of which their number has been greatly increased. The first was by Mrs. Fleming, who, in studying the photographs of the Henry Draper Memorial, found that the stars of the third type, in which the hydrogen lines are bright, are variables of long period. From this property she has discovered 128 new variables, and has also shown how they may be classified from their spectra. The differences between the first, second and third types of spectra are not so great as those between the spectra of different variables of long period. The second discovery is that of Professor Bailey, who found that certain globular clusters contain large numbers of variable stars of short period. He has discovered 509 new variables, 396 of them in four clusters. The third discovery, made by Professor Wolf, of Heidelberg, that variables occur in large nebulæ, has led to his discovery of 65 variables. By similar work, Miss Leavitt has found 295 new variables. The total number of variable stars discovered by photography during the last fifteen years is probably five times the entire number found visually up to the present time. Hundreds of thousands of photometric measures will be required to determine the light curves, periods and laws regulating the changes these objects undergo.

A far more comprehensive problem, and perhaps the greatest in astronomy, is that of the distribution of the stars, and the constitution of the stellar universe. No one can look at the heavens, and see such clusters as the Pleiades, Hyades and Coma Berenices, without being convinced that the distribution is not due to chance. This view is strengthened by the clusters and doubles seen in even a small telescope. We also see at once that the stars must be of different sizes, and that the faint stars are not necessarily the most distant. If the number of stars was infinite, and distributed according to the laws of chance throughout infinite and empty space, the background of the sky would be as bright as the surface of the sun. This is far from being the case. While we can thus draw general conclusions, but little definite information can be obtained, without accurate quantitative measures, and this is one of the greatest objects of stellar photometry. If we consider two spheres, with the sun as the common center, and one having ten times the radius of the other, the volume of the first will be one thousand times as great as that of the second. It will, therefore, contain a thousand times as many stars. But the most distant stars in the first sphere would be ten times as far off as those in the second sphere, and accordingly if equally bright would appear to have only one hundredth part of the apparent brightness. Expressed in stellar magnitudes, they would be five magnitudes fainter. In reality, the total number of stars of the fifth magnitude and brighter is about 1,500;

of the tenth magnitude, 373,000 instead of 1,500,000, as we should expect. An absorbing medium in space, which would dim the light of the more distant stars, is a possible explanation, but this hypothesis does not agree with the actual figures. An examination of the number of adjacent stars shows that it is far in excess of what would be expected if the stars were distributed by chance. Of the three thousand double stars in the 'Mensuræ Micrometricæ,' the number of stars optically double, or of those which happen to be in line, according to the theory of probabilities, is only about forty. This fact should be recognized in any conclusions regarding the motions of the fixed stars, based upon measures of their position with regard to adjacent bright stars.

We have here neglected all conclusions based upon the difference in composition of different stars. Photographs of their spectra furnish the material for studying this problem in detail. About half of the stars have spectra in which the broad hydrogen lines are the distinguishing feature. They are of the first type, and belong to class A of the classification of the Henry Draper Memorial. The Milky Way consists so completely of such stars that if they were removed it would not be visible. The Orion stars, forming class B, a subdivision of the first type in which the lines of helium are present, are still more markedly concentrated in the Milky Way. A large part of the other stars, forming one third of the whole, have spectra closely resembling that of the sun. They are of the second type, and form classes C and K. These stars are distributed nearly uniformly in all parts of the sky. Class M, the third type, follows the same law. Class F, whose spectrum is intermediate between classes A and C, follows the same law of distribution as classes G and K, but differs from them, if at all, in the opposite direction from class A. There, therefore, seem to be actually fewer of these stars in the Milky Way than outside of it. One class of stars, the fifth type, class O, has a very remarkable spectrum and distribution. A large part of the light is monochromatic. Of the ninety-six stars of this type so far discovered, twenty-one are in the Large Magellanic Cloud, one in the Small Magellanic Cloud, and the remainder follow the central line of the Milky Way so closely that the average distance from it is only two degrees. All of these stars, with the exception of sixteen, have been found by means of the Henry Draper Memorial.

It will be seen from the above discussion that stellar photometry in its broadest sense furnishes the means of attacking, and perhaps of solving, the greatest problem presented to the mind of man—the structure and constitution of the stellar universe, of which the solar system itself is but a minute and insignificant molecule.

THE FUNDAMENTAL CONCEPTS OF PHYSICAL SCIENCE.*

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ALL algebra, as was pointed out by von Helmholtz nearly fifty years ago, is based upon the three following very simple propositions:

Things equal to the same thing are equal to each other.

If equals be added to equals the wholes are equal.

If unequals be added to equals the wholes are unequal.

Geometry, he adds, is founded upon a few equally obvious and simple axioms.

The science of physics, similarly, has for its foundation three fundamental conceptions: those of *mass, distance* and *time,* in terms of which all physical quantities may be expressed.

Physics, in so far as it is an exact science, deals with the relations of these so-called physical quantities; and this is true not merely of those portions of the science which are usually included under the head of physics, but also of that broader realm which consists of the entire group of the physical sciences, viz., astronomy, the physics of the heavens; chemistry, the physics of the atom; geology, the physics of the earth's crust; biology, the physics of the matter imbued with life; physics proper (mechanics, heat, electricity, sound and light).

The manner in which the three fundamental quantities L, M and T (length, mass and time) enter, in the case of a physical quantity, is given by its *dimensional formula.*

Thus the dimensional formula for an acceleration is LT^{-2} which expresses the fact that an acceleration is a velocity (a length divided by a time) divided by a time. Energy has for its dimensional formula L^2MT^{-2}; it is a force, $LT^{-2}M$ (an acceleration multiplied by a mass), multiplied by a distance.

Not all physical quantities, in the present state of our knowledge, can be assigned a definite dimensional formula, and this indicates that not all of physics has as yet been reduced to a clearly established mechanical basis. The dimensional formula thus affords a valuable criterion of the extent and boundaries of our strictly definite knowledge of physics. Within these boundaries we are on safe and easy ground and are dealing, independent of all speculation, with the relations between precisely defined quantities. These relations are

* An address at the International Congress of Arts and Science, St. Louis, September, 1904.

† Von Helmholtz, *Populäre Wissenschaftliche Vorträge,* p. 136.

mathematical and the entire superstructure is erected upon the three fundamental quantities L, M and T and certain definitions; just as geometry arises from its axioms and definitions.

Of many of those physical quantities, for which we are not as yet able to give the dimensional formula, our knowledge is precise and definite, but it is incomplete. In the case, for example, of one important group of quantities, those used in electric and magnetic measurements, we have to introduce, in addition to L, M and T, a constant factor to make the dimensional formula complete. This, the *suppressed factor* of Rücker,* is μ, the magnetic permeability, when the quantity is expressed in the electromagnetic system, and becomes k, the specific inductive capacity, when the quantity is expressed in terms of the electro-static system.

Here the existence of the suppressed factor is indicative of our ignorance of the mechanics involved. If we knew in what way a medium like iron increased the magnetic field or a medium like glass the electric field, we should probably be able to express μ and k in terms of the three selected fundamental dimensions and complete the dimensional formulæ of a large number of quantities.

Where direct mechanical knowledge ceases the great realm of physical speculation begins. It is the object of such speculation to place all phenomena upon a mechanical basis; excluding as unscientific all occult, obscure and mystical considerations.

Whenever the mechanism, by means of which phenomena are produced, is incapable of direct observation either because of its remoteness in space, as in the case of physical processes occurring in the stars, or in time, as in the case of the phenomena with which the geologist has to do, or because of the minuteness of the moving parts, as in molecular physics, physical chemistry, etc., the speculative element is unavoidable. Here we are compelled to make use of analogy. We infer the unknown from the known. Though our logic be without flaw and we violate no mathematical principle, yet are our conclusions not absolute. They rest of necessity upon *assumptions*, and these are subject to modification indefinitely as our knowledge becomes more complete.

A striking instance of the uncertainties of extrapolation and of the precarious nature of scientific assumptions is afforded by the various estimates of the temperature of the sun. Pouillet placed this temperature between 1461° C. and 1761° C.; Secchi at 5,000,000°; Ericsson at 2,500,000°. The newer determinations,† of the temperature of the surface are, to be sure, in better agreement. Le Chatelier finds it to be 7600°; Paschen 5400°; Warburg 6000°. Wilson and Gray publish as their corrected result, 8000°. The estimate of the internal temperature is of a more speculative character. Schuster's computation gives

* Rücker, *Philos. Mag.*, 27, p. 104, 1889.

† See Arrhenius, *Kosmische Physik*, p. 131.

6,000,000° to 15,000,000°; that of Kelvin 200,000,000°; that of Ekholm 5,000,000°.

Another interesting illustration of the dangers of extrapolation occurs in the history of electricity. Faraday, starting from data concerning the variation between the length of electric sparks through air with the difference of potential, made an interesting computation of the potential difference between earth and sky necessary to discharge a cloud at a height of one mile. He estimated the difference of potential to be about 1,000,000 volts. Later investigations of the sparking distance have, however, shown this function to possess a character quite different from that which might have been inferred from the earlier work, and it is likely that Faraday's value is scarcely nearer the truth than was the original estimate of the temperature of the sun, mentioned above.

Still another notable instance of the errors to which physical research is subject when the attempt is made to extend results beyond the limits established by actual observation occurs in the case of the measurements of the infra-red spectrum of the sun by Langley. His beautiful and ingenious device, the bolometer, made it possible to explore the spectrum to wave-lengths beyond those for which the law of dispersion of the rock-salt prism had at that time been experimentally determined. Within the limits of observation the dispersion showed a curve of simple form, tending apparently to become a straight line as the wave-length increased. There was nothing in the appearance of the curve to indicate that it differed in character from the numerous empirical curves of similar type employed in experimental physics, or to lead even the most experienced investigator to suspect values for the wave-length derived from an extension of the curve. The wave-lengths published by Langley were accordingly accepted as substantially correct by all other students of radiation, but subsequent measurements of the dispersion of rock salt at the hands of Rubens and his coworkers showed the existence of a second sudden and unlooked-for turn of the curve just beyond the point at which the earlier determinations ceased; and in consequence Langley's wave-lengths and all work based upon them are now known to be not even approximately accurate. The history of physics is full of such examples of the dangers of extrapolation, or, to speak more broadly, of the tentative character of most of our assumptions in experimental physics.

We have then two distinct sets of physical concepts. The first of these deals with that positive portion of physics the mechanical basis of which, being established upon direct observation, is fixed and definite, and in which the relations are as absolute and certain as those of mathematics itself. Here speculation is excluded. Matter is simply one of the three factors which enters, by virtue of its mass, into our formulæ for energy, momentum, etc. Force is simply a quantity of which we need to know only its magnitude, direction, point of appli-

cation and the time during which it is applied. The Newtonian conception of force—the producer of motion—is adequate. All troublesome questions as to how force acts, of the mechanism by means of which its effects are produced are held in abeyance.

Speculative physics, to which the second set of concepts belong, deals with those portions of the science for which the mechanical basis has to be imagined. Heat, light, electricity and the science of the nature and ultimate properties of matter belong to this domain.

In the history of the theory of heat we find one of the earliest manifestations of a tendency so common in speculative physics that it may be considered characteristic; the assumption of a medium. The medium in this case was the so-called *imponderable* caloric; and it was one of a large class, of which the two electric fluids, the magnetic fluid, etc., were important members.

The theory of heat remained entirely speculative up to the time of the establishment of the mechanical equivalent of heat by Joule. The discovery that heat could be measured in terms of work, injected into thermal theory the conception of energy and led to the development of thermodynamics.

Generalizations of the sort expressed by Tyndall's phrase, *heat a mode of motion,* follow easily from the experimental evidence of the part which energy plays in thermal phenomena, but the specification of the precise mode of motion in question must always depend upon our views concerning the nature of matter, and can emerge from the speculative stage only, if ever, when our knowledge of the mechanics of the constitution of matter becomes fixed. The problem of the mechanism by which energy is stored or set free rests upon a similar speculative basis.

These are proper subjects for theoretical consideration, but the dictum of Rowland* that we get out of mathematical formulæ only what we put into them should never be lost from sight. So long as we put in only assumptions we shall take out hypotheses, and useful as these may prove, they are to be regarded as belonging to the realm of scientific speculation. They must be recognized as subject to modification indefinitely as we, in consequence of increasing knowledge, are led to modify our assumptions.

The conditions with which the physicist has to deal in his study of optics are especially favorable to the development of the scientific imagination, and it is in this field that some of the most remarkable instances of successful speculative work are to be found. The emission theory died hard and the early advocates of the undulatory theory of light were forced to work up with a completeness, probably without parallel in the history of science, the evidence, necessarily indirect, that in optics we have to do with a wave-motion. The standpoint of

* Rowland, president's address to the American Physical Society, 1900.

optical theory may be deemed conclusive, possibly final, so far as the general proposition is concerned that it is the science of a wave-motion. In a few cases, indeed, such as the photography of the actual nodes of a standing waves ystem, by Wiener, we reach the firm ground of direct observation.

Optics has nevertheless certain distinctly speculative features. Wave-motion demands a medium. The enormous velocity of light excludes known forms of matter; the transmission of radiation in vacuo and through outer space from the most remote regions of the universe, and at the same time through solids such as glass demands that this medium shall have properties very different from that of any substance with which chemistry has made us acquainted.

The assumption of a medium is, indeed, an intellectual necessity and the attempt to specify definitely the properties which it must possess in order to fulfill the extraordinary functions assigned to it has afforded a field for the highest display of scientific acumen. While the problem of the mechanism of the luminiferous ether has not as yet met with a satisfactory solution, the ingenuity and imaginative power developed in the attack upon its difficulties command our admiration.

Happily the development of what may be termed the older optics did not depend upon any complete formulation of the mechanics of the ether. Just as the whole of the older mechanics was built up from Kepler's laws, Newton's laws of motion, the law of gravitational attraction, the law of inverse squares, etc., without any necessity of describing the mechanics of gravitation or of any force, or of matter itself, so the system of geometrical relations involved in the consideration of reflection and refraction, diffraction, interference and polarization was brought to virtual completion without introducing the troublesome questions of the nature of the ether and the constitution of matter.

Underlying this field of geometrical optics or what I have just termed the older optics are, however, a host of fundamental questions of the utmost interest and importance, the treatment of which depends upon molecular mechanics and the mechanics of the ether. Our theories as to the nature and causes of radiation, of absorption and of dispersion, for example, belong to the newer optics and are based upon our conceptions of the constitution of matter; and since our ideas concerning the nature of matter, like our knowledge of the ether, is purely speculative, the science of optics has a doubly speculative basis. One type of selective absorption, for example, is ascribed to resonance of the particles of the absorbing substance, and our modern dispersion theories depend upon the assumption of natural periods of vibration of the particles of the refracting medium of the same order of frequency as that of the light waves. When the frequency of the waves falling upon a substance coincides with the natural period of vibration of the particles of the latter we have selective absorption, and accom-

panying it anomalous dispersion. For these and numerous other phenomena no adequate theory is possible which does not have its foundation upon some assumed conception as to the constitution of matter.

The development of the modern idea of the ether forms one of the most interesting chapters in the history of physics. We find at first a tendency to assume a number of distinct media corresponding to the various effects (visual, chemical, thermal, phosphorescent, etc.) of light waves, and later the growth of the conception of a single medium, the luminiferous ether.

In the development of electricity and magnetism, meantime, the assumption of media was found to be an intellectual necessity without which no definite philosophy of the phenomena was possible. At first there was the same tendency to a multiplicity of media—there were the positive and negative electric fluids, the magnetic fluid, etc. Then there grew up in the fertile mind of Faraday that wonderful fabric of the scientific imagination, the electric field; the conception upon which all later attempts to form an idea of a thinkable mechanism of electric and magnetic action have been established.

It is the object of science, as has been pointed out by Ostwald, to reduce the number of hypotheses, and the highest development would be that in which a single hypothesis served to elucidate the relations of the entire universe. Maxwell's discovery that the whole theory of optics is capable of expression in terms identical with those found most convenient and suitable in electricity, in a word that optics may be treated simply as a branch of electromagnetics, was the first great step towards such a simplification of our fundamental conceptions. This was followed by Hertz's experimental demonstration of the existence of artificially produced electromagnetic waves in every respect identical with light waves, an achievement which served to establish upon a sure foundation the conception of a single medium. The idea of one universal medium as the mechanical basis for all physical phenomena was not altogether new to the theoretical physicist but the unification of optics and electricity did much to strengthen this conception.

The question of the ultimate structure of matter, as has already been pointed out, is also speculative in the sense that the mechanism upon which its properties are based is out of the range of direct observation. For the older chemistry and the older molecular physics the assumption of an absolutely simple atom and of molecules composed of comparatively simple groupings of such atoms sufficed. Physical chemistry and that new phase of molecular physics which has been termed the physics of the ion demand the breaking up of the atom into still smaller parts and the clothing of these with an electric charge. The extreme step in this direction is the suggestion of Larmor that the electron is a 'disembodied charge' of negative electricity.

Since, however, in the last analysis, the only conception having a

definite and intelligible mechanical basis which physicists have been able to form of an electric charge is that which regards it as a phenomenon of the ether, this form of speculation is but a return under another name to views which had earlier proved attractive to some of the most brilliant minds in the world of science, such as Helmholtz and Kelvin. The idea of the atom, as a vortex motion of a perfect fluid (the ether), and similar speculative conceptions, whatever be the precise form of mechanism imagined, are of the same class as the moving electric charge of the later theorists.

Lodge* in a recent article in which he attempts to voice in a popular way the views of this school of thought says:

"*Electricity under strain constitutes 'charge'; electricity in locomotion constitutes light. What electricity itself is we do not know, but it may, perhaps, be a form or aspect of matter. . . . Now we can go one step further and say, matter is composed of electricity and of nothing else. . . .*"

If for the word *electricity* in this quotation from Lodge we substitute *ether,* we have a statement which conforms quite as well to the accepted theories of light and electricity as his original statement does to the newer ideas it is intended to express.

This reconstructed statement would read as follows:

Ether under strain constitutes 'charge'; *ether* in locomotion constitutes current and magnetism; *ether* in vibration constitutes light. What *ether* itself is we do not know, but it may, perhaps, be a form or aspect of matter. Now we can go one step further and say: Matter is composed of *ether* and of nothing else.

The use of the word electricity, as employed by Lodge and others, is now much in vogue, but it appears to me unfortunate. It would be distinctly conducive to clearness of thought and an avoidance of confusion to restrict the term to the only meaning which is free from criticism; that in which it is used to designate the science which deals with electrical phenomena.

The only way in which the noun *electricity* enters, in any definite and legitimate manner into our electrical treatises is in the designation of Q in the equations

$$Q = \int Idt,\ C = Q/E,\ W = QE, \text{ etc.}$$

Here we are in the habit—whether by inheritance from the age of the electric fluid, by reason of the hydrodynamic analogy or as a matter of convention or of convenience merely—of calling Q the *quantity of electricity.*

Now Q is 'charge' and its unit the coulomb is unit charge. The alternative expression, *quantity of electricity,* is a purely conventional designation and without independent physical significance. It owes its

* Lodge, *Harper's Magazine,* August, 1904, p. 383.

prevalence among electricians to the fact that by virtue of long familiarity we prefer to think in terms of matter, which is tangible, rather than of ether.

Charge is to be regarded as fundamental, and its substitute, quantity of electricity, as merely an artificial term of convenience; because of the former we have a definite mechanical conception, whereas we can intelligently define a quantity of electricity only in terms of *charge*.

In the science of heat the case differs in that the term heat is used, if not as precisely synonymous with energy, at least for a quantity having the same dimensions as energy and having as its unit the erg. It might easily have happened, as has happened in electrical theory, that the ancient notion of a *heat substance* should survive, in which case we should have had for the quantity of heat not something measured in terms of energy, but, as in the case of electricity, one of the terms which enter into our expression for energy. We should then have had to struggle continually, in thermo-dynamics, as we now do in electrical theory, against the tendency to revert to an antiquated and abandoned view.

It would, I can not but think, have been fortunate had the word electricity been used for what we now call electrical energy; using *charge*, or some other convenient designation, for the quantity Q. That aspect of the science in accordance with which we regard it as a branch of energetics in which movements of the ether are primarily involved would have been duly emphasized. We should have been quit forever of the bad notion of electricity as a medium, just as we are already freed from the incubus of heat as a medium. We should have had *electricity—a mode of motion* (or stress) of the ether as we have *heat—a mode of motion* (of matter). When our friends asked us: 'What is electricity?' we should have had a ready answer for them instead of a puzzled smile.

One real advance which has been attained by means of the theory of ionization, and it is of extreme significance and of far-reaching importance, consists in the discovery that electrification or the possession of charge, instead of being a casual or accidental property, temporarily imparted by friction or other process, is a fundamental property of matter. According to this newer conception of matter, the fruit of the ionic theory, the ultimate parts of matter, are electrically charged particles. In the language of Rutherford:*

> **It must then be supposed that the process of ionization in gases consists in a removal of a negative corpuscle or electron from the molecule of gas. At atmospheric pressure this corpuscle immediately becomes the center of an aggregation of molecules which moves with it and *is* the negative ion. After removal of the negative ion the molecule retains a positive charge and probably also becomes the center of a cluster of new molecules.**
>
> **The *electron* or *corpuscle* is the body of smallest mass yet known to science.**

* Rutherford, 'Radioactivity,' p. 53, 1904.

It carries a negative charge of 3.4×10^{-10} electrostatic units. Its presence has only been detected when in rapid motion, when it has for speeds up to about 10^{10} cms. a second, an apparent mass m given by $e/m = 1.86 \times 10^{7}$ electromagnetic units. This apparent mass increases with the speed as the velocity of light is approached.

At low pressures the electron appears to lose its load of clustering molecules, so that finally the negative ion becomes identical with the electron or corpuscle and has a mass, according to the estimates of J. J. Thomson, about one thousandth of that of the hydrogen atom. The positive ion is, however, supposed to remain of atomic size even at low pressures.

The ionic theory and the related hypothesis of electrolytic dissociation afford a key to numerous phenomena concerning which no adequate or plausible theories had hitherto been formed. By means of them explanations have been found, for example, of such widely divergent matters as the positive electric charge known to exist in the upper atmosphere and the perplexing phenomena of fluorescence.

The evidence obtained by J. J. Thomson and other students of ionization, that electrons from different substances are identical, has greatly strengthened the conviction which for a long time has been in process of formation in the minds of physicists, that all matter is in its ultimate nature identical. This conception, necessarily speculative, has been held in abeyance by the facts, regarded as established and lying at the foundation of the accepted system of chemistry, of the conservation of matter and the intransmutability of the elements. The phenomena observed in recent investigations of radioactive substances have, however, begun to shake our faith in this principle.

If matter is to be regarded as a product of certain operations performed upon the ether there is no theoretical difficulty about transmutation of elements, variation of mass or even the complete disappearance or creation of matter. The absence of such phenomena in our experience has been the real difficulty, and if the views of students of radioactivity concerning the transformations undergone by uranium, thorium and radium are substantiated, the doctrines of the conservation of mass and matter which lie at the foundation of the science of chemistry will have to be modified. There has been talk of late of violations of the principle of the conservation of energy in connection with the phenomena of radio-activity, but the conservation of matter is far more likely to lose its place among our fundamental conceptions.

The development of physics on the speculative side has led, then, to the idea, gradually become more definite and fixed, of a universal medium, the existence of which is a matter of inference. To this medium properties have been assigned which are such as to enable us to form an intelligible, consistent conception of the mechanism by means of which phenomena, the mechanics of which is not capable of direct observation, may be logically considered to be produced. The

great step in this speculation has been the discovery that a single medium may be made to serve not only for the numerous phenomena of optics, but, without ascribing to it any characteristics incompatible with a luminiferous ether, is equally available for the description and explanation of electric and magnetic fields, and finally may be made the basis for intelligible theories of the structure of matter.

To many minds this seemingly universal adaptability of the ether to the needs of physics almost removes it from the field of speculation; but it should not be forgotten that a system, entirely imaginary, may be devised, which fits all the known phenomena and appears to offer the only satisfactory explanation of the facts, and which subsequently is abandoned in favor of other views. The history of physics is full of instances where a theory is for a time regarded as final on account of its seeming completeness, only to give way to something entirely different.

In this consideration of the fundamental concepts I have attempted to distinguish between those which have the positive character of mathematical laws and which are entirely independent of all theories of the ultimate nature of matter and those which deal with the latter questions and which are essentially speculative. I have purposely refrained from taking that further step which plunges us from the heights of physics into the depths of philosophy.

With the statement that science in the ultimate analysis is nothing more than *an attempt to classify and correlate our sensations* the physicist has no quarrel. It is, indeed, a wholesome discipline for him to formulate for himself his own relations to his science in terms such as those which, to paraphrase and translate very freely the opening passages of his recent 'Treatise on Physics,' Chwolson* has employed.

> For every one there exist two worlds, an inner and an outer, and our senses are the medium of communication between the two. The outer world has the property of acting upon our senses, to bring about certain changes, or, as we say, to exert certain stimuli.
>
> The inner world, for any individual, consists of all those phenomena which are absolutely inaccessible (so far as direct observation goes) to other individuals. The stimulus from the outer world produces in our inner world a subjective perception which is dependent upon our *consciousness*. The subjective perception is made objective, viz., is assigned *time* and *place* in the outer world and given a name. The investigation of the processes by which this objectivication is performed is a function of philosophy.

Some such confession of faith is good for the man of science; *lest he forget;* but once it is made he is free to turn his face to the light once more, thankful that the *investigation of objectivication* is, indeed, *a function of philosophy* and that the only speculations in which he, as a physicist, is entitled to engage are those which are amenable at every step to mathematics and to the equally definite axioms and laws of mechanics.

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* Chwolson, 'Physik,' Vol. I., Introduction.

THE METHODS OF THE EARTH-SCIENCES.*

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IT is my assigned task to review the methods of the earth-sciences. The technical processes of the constituent sciences are peculiar to each and are inappropriate subjects for discussion before this composite assemblage; but the fundamental methods of intellectual procedure are essentially common to all the earth-sciences, and to these the address will confine itself.

That which passes under the name earth-science is not all *science* in the strict sense of the term. Not a little consists of generalizations from incomplete data, of inferences hung on chains of uncertain logic, of interpretations not beyond question, of hypotheses not fully verified and of speculation none too substantial. A part of the mass is true science, a part is philosophy, as I would use the term, a part is speculation, and a part is yet unorganized material. However, I like to think of the aggregate, not as an amorphous mixture of science, philosophy and speculation, but as a rather definite aggregation of these, not unlike that of the earth itself. The great mass of our subject-material may be regarded as a lithosphere of solid facts. Around this gathers an atmosphere of philosophy, rather dense near the contact zone, but thinning away into tenuous speculation in the outer regions. For myself, I like to think of the nucleus as solid and firm throughout, not as a thin fractured crust floating on a fiery liquid of plutonian suggestiveness. I like to think of the philosophic and speculative atmosphere as no mere gas-zone of forty-five miles' depth, as of old, but as an envelope of intense kinetic life, in the denser zone, where the logical molecules touch one another with marvelous frequency, and where there is frictional contact with the solid but rather inert lithosphere. In the outer tenuous zone, the molecular flights are freer and the excursions are without assignable limits. I believe an appropriate atmosphere of philosophy is as necessary to the wholesome intellectual life of our sciences as is the earth's physical atmosphere to the life of the planet. None the less, it must ever be our endeavor to reduce speculation to philosophy, and philosophy to science. For the perpetuation of the necessary philosophic atmosphere, we may safely trust to the evolution of new problems concurrently with the solution of the old.

But granting the importance of the philosophic element, we doubt-

* An address at the International Congress of Arts and Science, St. Louis, September, 1904.

less agree without hesitation that the solid products of accurate and complete observation, natural or experimental, are the bed-rock of our group of sciences. The first great object sought by laudable methods is, therefore, the promotion of the most accurate, searching, exhaustive and unbiased observation that is possible. One of the primary efforts in behalf of our sciences, therefore, was naturally directed to the task of promoting the best observational work. It was soon discovered that two chief dangers threatened the worker—bias and incompleteness. To guard against the first there was evolved

The Method of Colorless Observation.

Under its guidance, the observer endeavors to keep his mind scrupulously free from prepossessions and favored views. However tensely he may strain his observing powers to see what is to be seen, he seeks solely a record of facts uncolored by preferences or prejudices. To this end, he restrains himself from theoretical indulgence, and modestly contents himself with being a recorder of nature. He does not presume to be its interpreter and prophet. At length, in the office, he gathers his observations into an assemblage, with such inferences and interpretations as flow from them spontaneously, but even then he guards himself against the prejudices of theoretical indulgence.

Laudable as this method is in its avoidance of partiality, it is none the less seriously defective. No one who goes into the field with a mind merely receptive, or merely alert to see what presents itself, however nerved to a high effort, will return laden with all that might be seen. Only a part of the elements and aspects of complex phenomena present themselves at once to even the best observational minds. Some parts of the complex are necessarily obscure. Some of the most significant elements are liable to be unimpressive. These unobtrusive but yet vital elements will certainly escape observation unless it is forced to seek them out, and to seek them out diligently, acutely and intensely. To make a reasonably complete set of observations, the mind must not only see what spontaneously arrests its attention, but it must immediately draw out from what it observes inferences, interpretations, and hypotheses to promote further observations. It must at once be seen that if a given inference is correct, certain collateral phenomena must accompany it. If another inference be correct, certain other phenomena must accompany it. If still a third interpretation be the true one, yet other phenomena must be present to give proof of it. Once these suggestions have arisen, the observer seeks out the phenomena that discriminate between them, and, under such stimulus, phenomena that would otherwise have wholly escaped attention at once come into view because the eye has now been focused for them. It may be affirmed with great confidence that without the active and instantaneous use of these concurrent processes the observer will rarely, if ever, record the whole of any one set of significant ele-

ments, much less the whole of all sets. His record will contain incomplete parts of different sets of significant elements, *but no complete set of any one.* The obscure factors of each set are quite sure to be overlooked and the obtrusive factors of several sets indiscriminately commingled. The method of colorless observation is thus seriously defective in the completeness of its products, while it successfully guards them from bias.

Standing over against it, in strong contrast, is the method which at once endeavors to seek out and put together the phenomena that are thought to be significant. This leads promptly to the construction of a theory or an explanation which soon comes to guide the work and gives rise to

The Method of the Ruling Theory.

The chief effort here centers on an elucidation of phenomena, not on an exhaustive determination of the facts. Properly enough the crown of the work is the end, explanation is brought to the forefront and eagerly made the immediate end of endeavor. As soon as a phenomenon is presented, a theory of elucidation is framed. Laudable enough in itself, the theory is liable to be framed before the phenomena are fully and accurately observed. The elucidation is likely to embrace only the more obtrusive phenomena, not the full complement of the obtrusive and the unimpressive. The field is quite likely to present many repetitions of the leading phenomena and a theory framed to fit those that first arrest attention naturally fits the oft-recurrent phenomena of the same class. While there may be really no new evidence, nor any real test, nor any further inquiry into the grounds of the theory, its repeated application with seeming success leads insidiously to the delusion that it has been strengthened by additional investigation. Unconsciously then it begins to direct observation to the facts it so happily elucidates. Unconsciously the facts to which it gives no meaning become less impressive and fall into neglect. Selective observation creeps insidiously in and becomes a persistent habit. Soon also affection is awakened with its blinding influence. The authorship of an original explanation that seems successful easily begets fondness for one's intellectual child. This affection adds its alluring influence to the previous tendency toward an unconscious selection. The mind lingers with pleasure upon the facts that fall happily into the embrace of the theory, and feels a natural indifference toward those that assume a refractory or meaningless attitude. Instinctively, there is a special searching-out of phenomena that support the theory; unwittingly also there is a pressing of the theory to make it fit the facts and a pressing of the facts to make them fit the theory. When these biasing tendencies set in, the mind soon glides into the partiality of paternalism, and the theory rapidly rises to a position of control. Unless it happens to be the true one, all hope of the best results is gone. The defects of this method are obvious and grave.

It is safe to say, however, that under this method, with all its defects, many facts will be gathered that an observer of colorless attitude would have quite overlooked. The reverse may doubtless also be said. An effort to avoid the dangers at once of the colorless Scylla and the biasing Charybdis gave rise to

The Method of the Working Hypothesis.

This may be regarded as the distinctive feature of the methodology of the last century. This differs from the method of the ruling theory in that the working hypothesis is made a means of determining facts, not primarily a thesis to be established. Its chief function is the suggestion and guidance of lines of inquiry; inquiry not for the sake of the hypothesis, but for the sake of the facts and their final elucidation. The hypothesis is a mode rather than an end. Under the ruling theory, the stimulus is directed to the finding of facts for the support of the theory. Under the working hypothesis, the facts are sought for the purpose of ultimate induction and demonstration, the hypothesis being but a means for the more ready development of facts and their relations, particularly *their relations.*

It will be seen that the distinction is somewhat subtile. It is rarely if ever perfectly sustained. A working hypothesis may glide with the utmost ease into a ruling theory. Affection may as easily cling about a beloved intellectual child under the name of a working hypothesis as under any other, and may become a ruling passion. The moral atmosphere associated with the working hypothesis, however, lends some good influence toward the preservation of its integrity. The author of a working hypothesis is not presumed to father or defend it, but merely to use it for what it is worth.

Conscientiously followed, the method of the working hypothesis is an incalculable advance upon the method of the ruling theory, as it is also upon the method of colorless observation; but it also has serious defects. As already implied, it is not an adequate protection against a biased attitude. Even if it avoids this, it tends to narrow the scope of inquiry and direct it solely along the lines of the hypothesis. It undoubtedly gives acuteness, incisiveness and thoroughness in its own lines, but it inevitably turns inquiry away from other lines. It has dangers therefore akin to its predecessor, the ruling theory.

A remedy for these dangers and defects has been sought in

*The Method of Multiple Working Hypotheses.**

This differs from the method of the simple working hypothesis in that it distributes the effort and divides the affections. It is thus in some measure protected against the radical defects of the two previous methods. The effort is to bring up into distinct view every rational explanation of the phenomenon in hand and to develop into working

* In this sketch I have drawn freely upon my paper on 'The Method of Multiple Working Hypotheses,' *Journ. Geol.*, V., 1897.

form every tenable hypothesis of its nature, cause or origin, and to give to each of these a due place in the inquiry. The investigator thus becomes the parent of a family of hypotheses; and by his paternal relations to all is morally forbidden to fasten his affections unduly upon any one. In the very nature of the case, the chief danger that springs from affection is counteracted. Where some of the hypotheses have been already proposed and used, while others are the investigator's own creation, a natural tendency to bias arises, but the right use of the method requires the impartial adoption of all into the working family. The investigator thus at the outset puts himself in cordial sympathy and in the parental relations of adoption, if not of authorship, with every hypothesis that is at all applicable to the case under investigation. Having thus neutralized, so far as may be, the partialities of his emotional nature, he proceeds with a certain natural and enforced erectness of mental attitude to the inquiry, knowing well that some of the family of hypotheses must needs perish in the ordeal of crucial research, but with a reasonable expectation that more than one of them may survive, since it often proves in the end that several agencies were conjoined in the production of the phenomenon. Honors must often be divided between hypotheses. In following a single hypothesis, the mind is biased by the presumptions of the method toward a single explanatory conception. But an adequate explanation often involves the coordination of several causes. This is especially true when the research deals with complicated phenomena such as prevail in the field of the earth-sciences. Not only do several agencies often participate, but their proportions and relative importance vary from instance to instance in the same class of phenomena. The true explanation is therefore necessarily multiple, and often involves an estimate of the measure of participation of each factor. For this the simultaneous use of a full staff of working hypotheses is demanded. The method of the single working hypothesis is here incompetent.

The reaction of one hypothesis upon another leads to a fuller and sharper recognition of the scope of each. Every added hypothesis is quite sure to call forth into clear recognition neglected aspects of the phenomena. The mutual conflicts of hypotheses whet the discriminative edge of each. The sharp competition of hypotheses provokes keenness in the analytic processes and acuteness in differentiating criteria. Fertility in investigative devices is a natural sequence. If therefore an ample group of hypotheses encompass the subject on all sides, the total outcome of observation, of discrimination and of recognition of significance and relationship is full and rich.

Closely allied to the method of multiple working hypotheses is

The Method of Multiple Series.

In many of the more complex problems of the earth-sciences the basal facts are but imperfectly determined, *e. g.*, the rate of rise of

internal temperature, the rigidity of the earth's body, the thermal conductivity of the earth's interior, the amount of the earth's shrinkage, the extent of lateral thrust in the formation of folded mountains and many others, indeed most others. There is need to deal with these problems notwithstanding the imperfection of the basal data, for in many cases these must long remain imperfect. Moreover, there is need to treat these problems tentatively to determine what fundamental facts are really needed, how these can best be secured, and with what precision they must be determined. Preliminary trial may save much tedious and expensive experimentation. It is as foolish to cultivate sterile soil in science as in agriculture, and preliminary tests may show that given soils are necessarily sterile. In many cases, all the needs of the problem may be met by a multiple series of assumptions covering the full range of a probable fact. In most cases it is easy to see that the value of a given fundamental factor can not range beyond certain extremes on either hand. If a series of values ranging from the one extreme to the other be used simultaneously in the inquiry, the full range of results dependent on this factor may be covered. In some inquiries this serves as well as if the exact truth were known, for whatever the assignable value, certain deductions can not stand. In other cases, it will be shown that a very slight change in the value of the basal factor will wholly change the outcome, and hence that extremely accurate determinations must be made before any trustworthy solution can be reached. Expensive determinations in the first case are folly; very accurate determinations in the second are to be sought at any cost. Conclusions on imperfect data in the one case are perfectly safe; conclusions without precise determinations in the other are folly. It is to be hoped that, with the wider adoption of the method of multiple series, tables of serial determinations covering the data of the more vital phenomena of the earth-sciences will be constructed, as tables of physical constants now are.

The Method of Regenerative Hypotheses.

In the method of multiple hypotheses, the members of the group are used simultaneously and are more or less mutually exclusive, or even antagonistic. Supplementary to this method is the use of a *succession* of hypotheses related genetically to one another. In this the results of an inquiry under the first hypothesis give rise to the assumptions of the succeeding hypothesis. The precise conclusions of the first inquiry are not made the assumptions of the second, for the process would then be little more than repetitive, but the revelations and intimations, perhaps the incongruities and incompatibilities of the first results beget, by their suggestiveness, the basis of the second. The latter is the offspring of the former, but between parent and offspring there is mutation with an evolutionary purpose. A cruder first attempt generates a more highly organized and specialized work-

ing scheme fitted to the new state of knowledge developed. The method is specially applicable to elaborate inquiries, particularly those in which the premises are imperfect and a long logical chain is hung upon them. The discussions of our great fundamental conceptions furnish the best examples, chiefly examples of the lack of a systematic regenerative method. Among these, two general classes may be recognized, (1) those of a rather rigorous type, as, for a distinguished example, the researches of George Darwin on tidal reaction and the history of the earth and moon, and (2) those of a looser and sometimes rather metaphysical type, which I shall try to illustrate by the doctrine of determinism. In all cases, *assumptions* are made the basis of the procedure. Absolute premises are not available. Taking its start from these assumptions, the process pursues a long course and at the end conclusions of great import are often drawn. Usually the process rests there, and in this lies a serious shortcoming. It should give rise to a new process of a higher order. Not seldom, a critical study of the results will reveal features that were not recognized nor suspected in the original assumptions, though really there. Out of these revelations should grow new assumptions and a new process. The second conclusion may in like manner betray unsuspected qualities and these should beget still other assumptions, and so the procedure should continue until the field is exhausted.

To choose a specific illustration is not a little delicate, for to be most familiar it must be of the negative type, but I fear such an illustration is the only way to clearly convey the meaning here intended. I therefore venture to choose one so eminent and so admirable, even with its limitations, that any suggestion of shortcoming will in no wise dim the luster of a great achievement. In the classic investigations of George Darwin on tides and their astronomic consequences, a viscous earth is assumed as the starting-point, with properties such that the tidal protuberance is carried forward by the rotation of the earth to the point which gives the maximum effect on the motions of the earth and moon. These assumptions run potentially through the whole train of brilliant mathematical deduction. At the end of the inquiry, or if not of this particular inquiry, at least of collateral inquiries, the conclusion is reached that the earth is a rigid body comparable to steel. Between such a rigid body and such a viscous earth as was assumed at the outset of the inquiry, there is a seeming incongruity. This, under the regenerative method, suggests a new investigation on the assumption that the earth is a very rigid body, with the further assumption that it has high elasticity of form, such that its protuberance may perhaps not be carried forward to the degree previously postulated. These new assumptions are the more imperative, because they are supported by inquiries based on quite independent lines. In framing the new hypothesis an advance in detail and in organization is to be sought on the evolutionary principle already in-

dicated. If the earth as a whole is as rigid as steel, and the outer part is, as we know, formed of rock much less rigid than steel, the interior must be much more rigid than steel and there must be a differential distribution of rigidity. The new inquiry may then well start with the assumption of increasing rigidity toward the center. Postulating an earth so constituted, a first step of the regenerated inquiry might well be an effort to learn not only the amount of the tidal protuberance, but also the *position* of the protuberance, since its position is as essential as its amount in influencing the motions of the earth and moon. As a geologist I venture to entertain the belief that exhaustive inquiry on such regenerative lines would bring forth results in harmony with geological evidences, with which the well-known conclusions heretofore reached seem to be at fatal variance.

The earth-sciences are not purely physical sciences. They concern themselves with life and with mentality, as well as with rocks, ocean and atmosphere. Our group is exceptionally comprehensive in the range of its subjects. Our methods should hence be such as to encompass the whole field. They should give us ultimately a complete working system of thought relative to all the earth is or holds. In some sense the earth sciences must come to comprehend the essentials of all the sciences. At least as much as any other scientists we are interested in the fundamental assumptions of all the sciences and in their consistent application. To touch hastily this broader field, I choose a second illustration of the method of regenerative hypotheses from the relations between the assumptions of science and the conclusions of science.

As our working basis, we assume that our perceptions represent reality, when duly directed and corrected, but that error and illusion lurk on all sides and must be scrupulously avoided. We assume that we are capable of detecting error and of demonstrating truth; and that, as requisite means, we have choice, and some measure of volitional command over ourselves and over nature.

Starting thus with assumptions that embrace choice and the possibility of error, and going out into physical research, most of us have concluded that antecedents are followed rigorously by their consequents. Going out a step further into the chemico-biological field and noting the close interrelations between physical and vital phenomena, many of us have been led to a belief in their ultimate identity. Going out a step further into the mental field, not a few of us have concluded that an unvarying sequence of antecedents and consequents reigns here also. But this seems to contradict the assumptions with which we started. Our primary assumptions embraced choice, volitional control, and the alternative of reaching truth or falling into error according to our self-directed discrimination.

What is to be done in the face of this seeming contradiction? The method of regenerative hypotheses answers that a new set of assump-

tions begotten of the contradictory conclusion should be made the basis of a new inquiry, and if possible of a new working hypothesis. Instead of the usual assumption of choice, and of the possible alternative of reaching truth or falling into error, let the assumption be that all acts of the mind are parts of a rigorous chain of antecedents and consequents. Let it be assumed that no swerving from the predetermined sequences is possible, that every thought and every act follows its antecedents with absolute rigor, no real choice, or volition, or alternative between accuracy and error being possible.

Let this set of assumptions be tried as a working hypothesis. If investigation be possible under it, let such investigation cover the whole ground of what we call truth and error. Let a distinction be drawn between absolutely predetermined mental actions corresponding to truthfulness on the one hand, and falsity on the other, if this be possible, and out of the former let science be constructed and let it be shown *why it is science,* and let the latter be disposed of in some suitable way. In other words, let the doctrine of determinism *be put into workable form,* and carried into effect in all its applications, with every step true to the primary assumptions. If this can be done successfully, we shall have a wholly new working basis for the production of science, with new criteria of science. If it can not be done and the hypothesis of determinism is unworkable, let it be cast aside like any other unworkable hypothesis. Whatever metaphysicians may think of an unworkable scheme, scientific investigators may as well send it to the junk shop.

Huxley once delivered himself of an able exposition of determinism. It was severely criticized by a fellow countryman who seemed to Huxley to have dealt with him unjustly, and he poured out the vials of his rhetorical wrath upon him as only Huxley could. But if determinism be true, I do not see how Huxley's critic could have swerved by a turn of a phrase from what he wrote, and Huxley's wrath was not more consistent than that assigned to Xerxes when he lashed the stormy Hellespont because it thwarted his purpose. But in this I may be wholly wrong. *Let determinism prove itself by giving rise to a complete and systematic working hypothesis.*

Whether this can be done or not, let any other basal assumptions suggested by the inquiry be made the ground of like attempts and be developed into full working hypotheses, if possible, and so continue the effort until the whole field is covered. Let it be seen what can and what can not be put into the form of a working system.

In this second illustration of the method of regenerative hypotheses, I have touched questions not usually thought to belong to the earth-sciences. It is none the less true that they are basal to the earth-sciences, as they are to all science, and to all true philosophy as well. The earth-sciences are entitled to probe for their own bottom as well as other sciences, or any philosophy, and it is altogether wholesome that

they should do so. The most serious source of error in the development of the earth-sciences, in my judgment, is our relative neglect to probe fundamental conceptions and to recognize the extent to which they influence the most common observations and interpretations. We need a method of thought that shall keep us alive to these basal considerations. To this end I believe it to be conducive to soundness of intellectual procedure to regard our whole system of interpretation as but an effort to develop a consistent system of workable hypotheses. I think we should do well to abandon all claims that we are reaching absolute truth, in the severest sense of that phrase, and content ourselves with the more modest effort to work out a system of interpretation which shall approve itself in practise under such tests as human powers can devise. Wherein lie

The Basal Criteria of Our Sciences?

I believe they lie essentially in *the working quality.* Whatever conforms thoroughly to the working requirements of nature probably corresponds essentially to the absolute truth, though it may be much short of the full truth. That may be accepted, for the time being, as true which duly approves itself under all tests, *as though it were true.* Whenever it seems to fail under test in any degree, confidence is to be withdrawn in equal degree, and a rectification of conceptions sought. This may well hold for all conceptions, however fundamental, whether they relate to the physical, the vital, or the mental phenomena which the earth presents. Let us entirely abandon the historic effort of the metaphysicians to build an inverted pyramid on an apex of axioms assumed to be incontestable truth, and let us rear our superstructure on the results of working trials applied as widely and as severely as possible. Let us seek our foundation in the broadest possible contact with phenomena. I hold that *the working test* when brought to bear in its fullest, most intimate and severest forms is *the supreme criterion* of that which should stand to us for truth. Our interpretative effort should, therefore, be to organize a complete set of working hypotheses for all phenomena, physical, vital and mental, so far as appropriate to our sphere of research. These should be at once the basis of our philosophy and of our science. These hypotheses should be constantly revised, extended and elaborated by all available means, and should be tested continually by every new relation which comes into view, until the crucial trials shall become as the sands of the sea for multitude and their severity shall have no bounds but the limits of human capacity. That which under this prolonged ordeal shall give the highest grounds of assurance may stand to us for science, that which shall rest more upon inference than upon the firmer modes of determination may stand to us for our philosophy, while that which lies beyond these, as something doubtless always will, may stand to us for the working material of the future.

UTILITARIAN SCIENCE.*

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IT falls to my lot to-day to discuss very briefly, in accordance with the program of this congress, some of the common features of utilitarian science, with a word as to present and future lines of investigation or instruction in some of those branches of the applications of knowledge which have been assigned to the present division.

Applied science can not be separated from pure science, for pure science may develop at any quarter the greatest and most unexpected economic values, while, on the other hand, the applications of knowledge must await the acquisition of knowledge, before any high achievement in any quarter can be reached. For these reasons, the classification adopted in the present congress, or any other classification of sciences into utilitarian science and other forms of science, must be illogical and misleading. Whatever is true is likely sometime to prove useful, and all error is likely to prove sometime disastrous. From the point of view of the development of the human mind, all truth is alike useful and all error is alike mischievous.

In point of development pure science must precede utilitarian science. Historically, this seems to be not true; for the beginnings of science in general, as alchemy, astrology and therapeutics, seem to have their origin in the desire for the practical results of knowledge. Men wanted to acquire gold, to save life, to forecast the future, not for knowledge's sake, but for the immediate results of success in these directions. But even here accurate knowledge must precede any success in its application, and accuracy of knowledge is all that we mean by pure science. Moreover, as through the ages the representatives of the philosophies of the day, the *a priori* explanations of the universe, were bitterly and personally hostile to all inductive conclusions based on the study of base matter, men of science were forced to disguise their work under a utilitarian cloak. This is more or less true even to this day, and the greatest need of utilitarian science is still, as a thousand years ago, that this cloak should be thrown off, and that a larger and stronger body of workers in pure science should be developed to give the advance in real knowledge on which the thousands of ingenious and noble applications to utilitarian ends must constantly depend.

It is a fundamental law of psychology that thought tends to pass over into action. Applied science is knowledge in action. It is the

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flower of that highest philanthropy of the ages by which not even thought exists for itself alone, but must find its end in the enlargement of human control over matter and force or the amelioration of the conditions of human life.

The development of all science has been a constant struggle, a struggle of fact against philosophy, of instant impressions against traditional interpretations, of truth against 'make-believe.' For men are prone to trust a theory rather than a fact; a fact is a single point of contact; a theory is a circle made of an infinite number of points, none of them, however, it may be, real points of contact.

The history of the progress of science is written in human psychology rather than in human records. It is the struggle of the few realities or present sense impressions against the multitude of past impressions, suggestions and explanations. I have elsewhere said that the one great discovery of the nineteenth century—forestalled many ages before—was that of the reality of external things. Men have learned to trust a present fact or group of facts, however contradictory its teachings, as apposed to tradition and philosophy. From this trust in the reality of the environment of matter and force, whatever these may be, the great fabric of modern science has been built up. Science is human experience of contact with environment tested, set in order, and expressed in terms of other human experience. Utilitarian science is that part of all this knowledge which we can use in our lives, in our business. What is pure science to one is applied science to another. The investigation of the laws of heredity may be strictly academic to us of the university, but they are utilitarian as related to the preservation of the nation or to the breeding of pigs. In the warfare of science the real in act and motive has been persistently substituted for the unreal. Men have slowly learned that the true glory of life lies in its wise conduct, in the daily act of love and helpfulness, not in the vagaries fostered by the priest or in the spasms of madness which are the culmination of war. To live here and now as a man should live constitutes the ethics of science, and this ideal has been in constant antithesis to the ethics of ecclesiasticism, of asceticism and of militarism.

The physical history of the progress of science has been a struggle of thinkers, observers and experimenters against the dominant forces of society. It has been a continuous battle, in which the weaker side in the long run is winner, having the strength of the earth behind. It has been incidentally a conflict of earth-born knowledge with opinions of men sanctioned by religion; of present fact with preestablished system, visibly a warfare between inductive thought and dogmatic theology.

The real struggle, as already indicated, lies deeper than this. It is the effort of the human mind to relate itself to realities in the midst of traditions and superstitions, to realize that Nature never contradicts herself, is always complex, but never mysterious. As a final result all

past systems of philosophy, perhaps all possible systems, have been thrown back into the realm of literature, of poetry, no longer controlling the life of action, which rests on fact.

This conflict of tendencies in the individual has become a conflict among individuals as each is governed by a dominant impulse. The cause of tradition becomes that of theology;—for men have always claimed a religious sanction for their own individual bit of cosmic philosophy. Just as each man in his secret heart, the center of his own universe, feels himself in some degree the subject of the favor of the mysterious unseen powers, so does society in all ages find a mystic or divine warrant for its own attitude towards life and action, whatever that may be.

The nervous system of man, inherited from that of the lower animals, may be regarded as primarily a means of making locomotion safe. The reflex action of the nerve center is the type of all mental processes. The sensorium, or central ganglion, receives impressions from the external world representing, in a way, various phases of reality. The brain has no source of knowledge other than sensation. All human knowledge comes through human experience. The brain, sitting in darkness, has the primary function of converting sensory impressions into impulses to action. To this end the motor nerves carry impulses outward to the muscles. The higher function of nerve action, which we call the intellect, as distinguished from simple reflex action and from instinct, is the choice among different responses to the stimulus of external realities. As conditions of life become more complex, the demands of external realities become more exacting. It is the function of the intellect to consider and of the mind to choose. The development of the mind causes and permits complexity in external relations. Safety in life depends on choosing the right response to external stimulus. Wrong choice leads to failure or to death.

From the demands of natural selection results the intense practicality of the mental processes. Our senses tell us the truth as to external nature, in so far as such phases of reality have been essential to the life of our ancestors. To a degree, they must have seen 'things as they really are,' else they should not have lived to continue the generation. Our own individual ancestors through all the ages have been creatures of adequate accuracy of sensation and of adequate power of thought. Were it not so they could not have coped with their environment. The sensations which their brains translated into action contained enough of absolute reality to make action safe. That our own ordinary sensations and our own inductions from them are truthful in their essentials, is proved by the fact that we have thus far safely trusted them. Science differs from common sense mainly in the perfection of its tools. That the instruments of precision used in science give us further phases of reality is shown by the fact that we can trust

our lives to them. We find it safer to do so than to trust our unaided senses.

While our senses tell us the truth as to familiar things, as rocks and trees, foods and shelter, friends and enemies, they do not tell us the whole truth: they go only so far as the demands of ancestral environment have forced them to go. Chemical composition our senses do not show. Objects too small to handle are too small to be seen. Bodies too distant to be reached are never correctly apprehended. Accuracy of sense decreases as the square of the distance increases. Sun and stars, clouds and sky, are in fact very different from what they seem to the senses.

In matters not vital to action, exactness of knowledge loses its importance. Any kind of belief may be safe, if it is not to be carried over into action. It is perfectly safe, in the ordinary affairs of life, for one who does not propose to act on his convictions to believe in witches and lucky stones, imps and elves, astral bodies and odic forces. It is quite as consistent with ordinary living to accept these as objective realities as it is to have the vague faith in microbes and molecules, mahatmas and protoplasm, protective tariffs and manifest destiny, which forms part of the mental outfit of the average American citizen to-day. Unless these conceptions are to be brought into terms of personal experience, unless in some degree we are to trust our lives to them, unless they are to be wrought into action, they are irrelevant to the conduct of life. As they are tested by action, the truth is separated from the falsehood, and the error involved in vague or silly ideas becomes manifest. As one comes to handle microbes, they become as real as bullets or oranges and as susceptible of being manipulated. But the astral body covers only ignorance and ghosts vanish before the electric light.

Memory pictures likewise arise to produce confusion in the mind. The record of past realities blends readily with the present. Men are gregarious creatures and their speech gives them the power to add to their own individual experiences the concepts and experiences of others. Suggestion and conventionality play a large part in the mental equipment of the individual man.

About the sense impressions formed in his own brain each man builds up his own subjective universe. Each accretion of knowledge must be cast more or less directly in terms of previous experience. By processes of suggestion and conventionality the ideas of the individual become assimilated to those of the multitude. Thus myths arise to account for phenomena not clearly within the ordinary experiences of life. And in all mythology the unknown is ascribed not to natural forces, but to the action of the powers that transcend nature, that lie outside the domain of the familiar and the real.

It has been plain to man in all ages that he is surrounded by forces stronger than himself, invisible and intangible, inscrutable in

their real nature, but terribly potent to produce results. He can not easily trace cause and effect in dealing with these forces; hence it is natural that he should doubt the existence of relations of cause and effect. As the human will seems capricious because the springs of volition are hidden from observation, so to the unknown will that limits our own we ascribe an infinite caprice. All races of men capable of abstract thought have believed in the existence of something outside themselves whose power is without human limitations. Through the imagination of poets the forces of nature become personified. The existence of power demands corresponding will. The power is infinitely greater than ours; the sources of its action inscrutable: hence man has conceived the unknown first cause as an infinite and unconditioned man. Anthropomorphism in some degree is inevitable, because each man must think in terms of his own experience. Into his own personal universe, all that he knows must come.

Recognition of the hidden but gigantic forces in nature leads men to fear and to worship them. To think of them either in fear or in worship is to give them human forms.

The social instincts of man tend to crystallize in institutions; even his common hopes and fears. An institution implies a division of labor. Hence, in each age and in each race men have been set apart as representatives of these hidden forces and devoted to their propitiation. These men are commissioned to speak in the name of each god that the people worship or each demon the people dread.

The existence of each cult of priests is bound up in the perpetuations of the mysteries and traditions assigned to their care. These traditions are linked with other traditions and with other mystic explanations of uncomprehended phenomena. While human theories of the sun, the stars, the clouds, of earthquakes, storms, comets and disease, have no direct relation to the feeling of worship, they can not be disentangled from it. The uncomprehended, the unfamiliar and the supernatural are one and the same in the untrained human mind; and one set of prejudices can not be dissociated from the others.

To the ideas acquired in youth we attach a sort of sacredness. To the course of action we follow we are prone to claim some kind of mystic sanction; and this mystic sanction applies not only to acts of virtue and devotion, but to the most unimportant rites and ceremonies; and in these we resent changes with the full force of such conservatism as we possess.

It is against limited and preconceived notions that the warfare of science has been directed. It is the struggle for the realities on the part of the individual man. Ignorance, prejudice and intolerance, in the long run, are one and the same thing. In some one line at least, every lofty mind throughout the ages has demanded objective reality. This struggle has been one between science and theology only because theological misconceptions were entangled with crude notions of other

sorts. In the experience of a single human life there is little to correct even the crudest of theological conceptions. From the supposed greater importance of religious opinions in determining the fate of men and nations, theological ideas have dominated all others throughout the ages; and in the nature of things, the great religious bodies have formed the stronghold of conservatism against which the separated bands of science have hurled themselves, seemingly, in vain.

But the real essence of conservatism lies not in theology. The whole conflict, as I have already said, is a struggle in the mind of man. From some phase of the warfare of science no individual is exempt. It exists in human psychology before it is wrought in human history. There is no better antidote to bigotry than the study of the growth of knowledge. There is no chapter in history more encouraging than that which treats of the growth of open mindedness. The study of this history leads religious men to avoid intolerance in the present, through a knowledge of the evils intolerance has wrought in the past. Men of science are spurred to more earnest work by the record that through the ages objective truth has been the final test of all theories and conceptions. All men will work more sanely and more effectively as they realize that no good to religion or science comes from 'wishing to please God with a lie.'

It is the mission of science to disclose—so far as it goes—the real nature of the universe. Its function is to eliminate, wherever it be found, the human equation. By methods of precision of thought and instruments of precision of observation and experiment, science seeks to make our knowledge of the small, the distant, the invisible, the mysterious as accurate, as practical, as our knowledge of common things. Moreover, it seeks to make our knowledge of common things accurate and precise, that this accuracy and precision may be translated into action. For the ultimate end of science as well as its initial impulse is the regulation of human conduct. Seeing true means thinking right. Right thinking means right action. Greater precision in action makes higher civilization possible. Lack of precision in action is the great cause of human misery; for misery is the inevitable result of wrong conduct. 'Still men and nations reap as they have strewn.'

A classic thought in the history of applied science is expressed in these words of Huxley: 'There can be no alleviation of the sufferings of man except in absolute veracity of thought and action, and a resolute facing of the world as it is.' 'The world as it is,' is the province of science. 'The God of the things as they are, is the God of the highest heaven.' And as to the sane man, the world as it is is glorious, beautiful, harmonious and divine, so will science, our tested and ordered knowledge of it, be the inspiration of art, poetry and religion.

Pure science and utilitarian science merge into each other at every point. They are one and the same thing. Every new truth can be

used to enlarge human power or to alleviate human suffering. There is no fact so remote as to have no possible bearing on human utility. Every new conception falls into the grasp of that higher philanthropy which rests on the comprehension of the truths of science. For science is the flower of human altruism. No worker in science can stand alone. None counts for much who tries to do so. He must enter into the work of others. He must fit his thought to theirs. He must stand on the shoulders of the past, and must crave the help of the future. The past has granted its assistance, to the fullest degree of the most perfect altruism. The future will not refuse; and, in return, whatever knowledge it can take for human uses, it will choose in untrammeled freedom. The sole line which sets off utilitarian science lies in the limitation of human strength and of human life. The single life must be given to a narrow field, to a single strand of truth, following it wherever it may lead. Some must teach, some must investigate, some must adapt to human uses. It is not often that these functions can be united in the same individual. It is not necessary that they should be united; for art is long, though life is short, and for the next thousand years science will be still in its infancy. We stand on the threshold of a new century; a century of science; a century whose discoveries of reality shall far outweigh those of all centuries which have preceded it; a century whose glories even the most conservative of scientific men dare not try to forecast. And this twentieth century is but one—the least, most likely—of the many centuries crowding to take their place in the line of human development. In each century we shall see a great widening of the horizon of human thought, a great increase of precision in each branch of human knowledge, a great improvement in the conditions of human life, as enlightenment and precision come to be controlling factors in human action.

In the remaining part of this address I shall discuss very briefly some salient features of practice, investigation and instruction in those sciences which in the scheme of classification of this congress have been assigned to this division. In this discussion I have received the invaluable aid of a large number of my colleagues in scientific work, and from their letters of kindly interest I have felt free to make some very interesting quotations. To all these gentlemen (a list too long to be given here) from whom I have received aid of this kind I offer a most grateful acknowledgment.

Engineering.

The development of the profession of engineering in America has been the most remarkable feature of our recent industrial as well as educational progress. In this branch of applied science our country has come to the very front, and this in a relatively short time. To this progress a number of distinct forces have contributed. One lies in the temperament of our people, their motive force, and their tendency to

apply knowledge to action. In practical life the American makes the most of all he knows. Favoring this is the absence of caste feeling. There is no prejudice in favor of the idle man. Only idlers take the members of the leisure class seriously. There is, again, no social discrimination against the engineer as compared with other learned professions. The best of our students become working engineers without loss of social prestige of any sort. Another reason is found in the great variety of industrial openings in America, and still another in the sudden growth of American colleges into universities, and universities in which both pure and applied sciences find a generous welcome. For this the Morrill Act, under which each state has developed a technical school, under federal aid, is largely responsible. In the change from the small college of thirty years ago, a weak copy of English models, to the American university of to-day, many elements have contributed. Among these is the current of enlightenment from Germany, and at the same time the influence of far-seeing leaders in education. Notable among these have been Tappan, Eliot, Agassiz and White. To widen the range of university instruction so as to meet all the intellectual, esthetic and industrial needs of the ablest men is the work of the modern university. To do this work is to give a great impetus to pure and to applied science.

Two classes of men come to the front in the development of engineering: the one, men of deep scientific knowledge, to whom advance of knowledge is due, the other the great constructive engineers; men who can work in the large and can manage great enterprises with scientific accuracy and practical success. Everywhere the tendency in training is away from mere craftsmanship and towards power of administration. The demands of the laboratory leave less and less time for the shop. "Two classes of students," says a correspondent, "should be encouraged in our universities: First, the man whose scientific attainments are such that he will be able to develop new and important processes, the details of which may be directly applied. This type of man is the scientific engineer. The other is the so-called practical man, who will not only actually carry on engineering work, but may be called on to manage large enterprises. If his temperament and ability are such as to give him a thorough command of business methods and details, while he is in addition a good engineer, he will find a field of great usefulness before him on leaving the university. The university should encourage young men to undertake the general executive work necessary to handling men and in the many details of large enterprises. The successful man of this character is necessarily a leader and the university should recognize that such a man can be of great influence in the world, if he is thoroughly and broadly educated."

"We need," says another correspondent, "men possessing a better general training than most of those now entering and leaving our engi-

neering schools. We need more thoroughly trained teachers of engineering, men who combine theoretical training with a wide and constantly increasing experience, men who can handle the factors of theory, practice and economics."

"Technical education," says another correspondent, "should look beyond the individual to the aggregate, and should aim to shape its activities so as to develop at the maximum number of points sympathetic and helpful relations with the industrial and engineering interests of the state. This means careful and steady effort towards the coordination of the activities of the technical school with the general condition of industry and engineering as regards its raw materials, its constructive and productive operations, its needs and demands with regard to personnel and its actual or potential trend of progress."

The coming era in engineering is less a period of discovery and invention than of application on a large scale of principles already known. Greater enterprises, higher potentialities, freer use of forces of nature, all these are in the line of engineering progress.

"The realm of physical science," says a correspondent, "has become to the practical man a highly improved agricultural land, whereas in earlier days it was a virgin country possessing great possibilities and exacting but little in the way of economic treatment."

In all forms of engineering, practise is changing from day to day; the principles remain fixed. In electricity, for example, the field of knowledge 'extends far beyond the direct limits or needs of electrical engineers.'

"The best criticism as to engineering education came formerly almost entirely from professors of science and engineering. To-day the greatest and most wholesome source of such criticism comes from those engaged in practical affairs. We have begun a regime wherein coordinated theory and practise will enter into the engineering training of young men to a far greater and more profitable extent than ever before."

"The marvelous results in the industrial world of to-day," says a correspondent, "are due largely to the spirit of 'usefulness, activity, and cooperation' that exists in each community of interests and which actuates men employing the means which applied science has so bountifully accorded. I know of no greater need of engineering education in our country to-day than that its conduct in each institution should be characterized by the same spirit of usefulness, activity and cooperation."

In mining, as in other departments of engineering, we find in the schools the same growing appreciation of the value of training at once broad, thorough and practical, and the same preference for the university-trained engineer in preference to the untrained craftsman.

The head of a great mining firm in London writes me that 'for our business, what we desire are young men of good natural qualifica-

tions, thoroughly trained theoretically without any so-called practical knowledge unless this knowledge has been gained by employment in actual works.'

On the pay-roll of this English firm I find that five men receive salaries of more than $20,000. All these are graduates of technical departments of American universities. Seventeen receive from $6,000 to $20,000. Nine of these were trained in American universities, one in Australia and two in England, while five have risen from the ranks.

In the lower positions, most have been trained in Australia, a few in England, while in positions bearing a salary of less than $2,500, most have risen from the ranks.

"Given men of equal qualifications," says the director of this firm, "the man of technical training is bound to rise to the higher position because of his greater value to his employer. As a rule, also, men who have been technically trained are, by virtue of their education, men who are endowed with a professional feeling which does not to the same extent exist among those men who have risen from the rank and file. They are therefore more trustworthy and especially in mining work, where premium for dishonesty exists, for this qualification alone they are bound to have precedence. We do not by any means wish to disparage the qualifications of many men who have risen from the ranks to eminent positions, but our opinion may be concentrated in the statement that even these men would be better men had they received a thorough technical training."

The progress of chemical engineering is parallel with that in other departments of technology. Yet the appreciation of the value of theoretical training is somewhat less marked, and in this regard our manufacturers seem distinctly behind those of Germany.

"The development of chemical industries in the past history of the United States," says a correspondent, "was seriously delayed by the usually superficial and narrow training of the chemist in the colleges. Thus managers and proprietors came to undervalue the importance of chemical knowledge. The greatest need at present in the development of chemical industries is an adequate supply of chemists of thorough training to teach manufacturers the importance in their business of adequate chemical knowledge. Epoch-making advances in chemical industry will spring from the brain of great chemists, and to ensure the production of a few of these, the country must expect to seed lavishly and to fertilize generously the soil from which they spring. Germany has learned the lesson well: other nations can not long delay."

In the vast range of the applications of science to agriculture, the same general statements hold good. There is, however, no such general appreciation of the value of training as appears in relation to the various branches of training, and the men of scientific education are mostly absorbed in the many ramifications of the Department of Agriculture and in the state agricultural colleges and experiment stations.

There are few illustrations of the power of national cooperation more striking than those shown in the achievements of the Department of Agriculture. I have no time to touch on the varied branches of agricultural research, the study of the chemistry of foods and soils, the practise of irrigation, the fight against adulterations, the fight against noxious insects, and all the other channels of agricultural art and practise. I can only commend the skill and the zeal with which all these lines of effort have been followed.

The art of agriculture is the application of all the sciences. Yet 'agricultural education,' writes a correspondent, 'has not yet reached the dignity of other forms of technical education.'

"The endowment of the science of agricultural research in the United States is greater than in any other country. The chief fault to be found is in striving too rapidly for practical applications and in not giving time enough for the fundamental research on which these applications must rest. The proportion of applied agricultural science in agriculture is too great in this country. While we do not need fewer workers in applied agricultural science, we do need more workers who would devote themselves to fundamental research."

Two branches of applied science not specifically noticed in our scheme of classification seem to me to demand a word of notice. One is selective breeding of plants and animals; the other, the artificial hatching of fishes. By the crossing of animals or plants not closely related, a great range of variety appears in the progeny. Some of these may have one or more of the desirable qualities of either parent. By selection of those possessing such qualities a new race may be formed in a few generations. The practical value of the results of such experiments can not be over-estimated. Although by no means a modern process, the art of selective breeding is still in its infancy. Its practise promises to take a leading place among the economically valuable applications of science. At the same time, the formation of species of organisms under the hand of man throws constant floods of light on the great questions of heredity, variation and selection in nature, the problem of the origin of species.

In this connection I may refer to artificial hatching and acclimatization of fishes, the work of the United States Bureau of Fisheries and of the fish commissions of the different states. There are many species of fish, notably those of the salmon family, in which the eggs can be taken and fertilized by artificial processes. These eggs can be hatched in protected waters so that the young will escape many of the vicissitudes of the brook and river, and a thousand young fishes can be sent forth where only a dozen grew before.

Medicine.

In the vast field of medicine I can only indicate in a few words cer-

tain salient features of medical research, of medical practise, and of medical instruction in America.

In matters of research, the most fruitful line of investigation has been along the line of the mechanism of immunity from contagious diseases. To know the nature of microorganisms and their effect on the tissues is to furnish the means of fighting them. 'The first place in experimental medicine to-day,' says Dr. W. H. Welch, 'is occupied by the problem of immunity.' That medicine is becoming a scientific profession and not a trade is the basis of the growing interest of our physicians in scientific problems, and this again leads to increased success in dealing with matters of health and disease. The discovery of the part played by mosquitoes in the dissemination of malaria, yellow fever, dengue, elephantiasis and other diseases caused by microorganisms marks an epoch in the study of these diseases. The conquest of diphtheria is another of the features of advance in modern medicine, and another is shown in the great development of surgical skill characteristic of American medical science. But the discoveries of the last decades have been rarely startling or epoch-making. They have rather tended to fill the gaps in our knowledge, and there remain many more gaps to fill before medical practise can reach the highest point of adequacy. The great need of the profession is still in the direction of research, and research of the character which takes the whole life and energy of the ablest men demands money for its maintenance. We need no more medical colleges for the teaching of the elements. We need schools or laboratories of research for the training of the masters.

In the development of medicine there has been a steady movement away from universal systems and *a priori* principles, on the one hand, and, on the other hand, from blind empiricism, with the giving of drugs with sole reference to their apparent results. The applications of science—all sciences which deal with life, with force and with chemical composition—must enter into the basis of medicine. Hence the insistent demand for better preliminary training before entering on the study of medicine. "Only the genius of the first order," says a correspondent, "can get on without proper schooling in his youth. What our medical investigators in this country most need is a thorough grounding in the sciences, especially physics and chemistry."

The instruction in medicine, a few years ago almost a farce in America, has steadily grown more serious. Laboratory work and clinical experience have taken the place of lectures, the courses have been lengthened, higher preparation for entrance has been exacted, though in almost all our schools these requirements are still far too low, and a more active and original type of teacher has been in demand. Even yet, so far as medical instruction is concerned, the hopeful sign is to be found in progress rather than in achievement. A college course, having as its major subjects the sciences fundamental to

medicine, is not too much to exact of a student who aspires to be a physician worthy of our times and of the degree of our universities. First hand knowledge of real things should be the key-note of all scientific instruction. "Far more effort is now made," writes a correspondent, "in both the preparatory and the clinical branches to give the student a first hand knowledge of the subject. This tendency has still a long way to travel before it is in danger of being overdone. The practical result of this tendency is that the cost of education per student is greatly increased and the profits of purely commercial schools are thereby threatened. This forms, doubtless, the main source of the objection made by the weaker and less worthy schools to better methods of instruction. We need well endowed schools of medicine that may carry on their work unhampered by the necessities of a commercial venture. Medical schools now exist in great numbers,—many of them can not keep up with modern requirements, and necessarily their salvation lies in antagonizing everything in the nature of more ample and more expensive training."

Another correspondent writes, emphasizing the value of biologic studies: "The final comprehension of bodily activity in health and disease depends on knowledge of living things from ovum to birth, from birth to maturity, and from maturity to old age and death. Anything less than such fundamental knowledge requires constant guessing to fill up the gaps, and guesses are nearly always wrong."

In many regards, even our best schools of medicine seem to show serious deficiencies. The teaching of anatomy is still one of the most costly, as well as least satisfactory, of our lines of work. A correspondent calls attention to the fact that in making anatomy 'practical' in our medical schools 'we expended last year $750,000 in the United States, twice the amount expended in Germany, with as a result neither practical anatomy nor scientific achievement.' "Anatomy," he continues, "should be made distinctly a university department, on a basis similar to that of physics and chemistry. Unfortunately, university presidents still stand much in the way of the development of anatomy, for many of them seem to think that almost any one who wears the gown is good enough to become a professor of anatomy. Repeatedly have I witnessed the appointment of a know-nothing when a recognized young man might have been had for half the money." Our forces are dissipated, the fear of things scientific has destroyed even the practical in this noble old mother science which is still giving birth to new sciences and to brilliant discoveries.

Among other matters too much neglected are personal hygiene, a matter to which the physician of the past has been notoriously and joyously indifferent. Especially is this true as regards the hygiene of exercise and the misuse of nerve-affecting drugs.

Public sanitation as well deserves more attention. "The demand for adequately trained officers of public health is not what it should be,

and our public service as a whole is far below that of European countries. Both public opinion and university authorities are responsible for this condition."

The hygiene of childhood, in which line great advances are made, is still not adequately represented in most of our medical colleges, and the study of psychiatry and nervous disturbances in general is not sufficiently lifted from the realm of quackery. "Not only," says a correspondent, "should psychiatry be taught in every medical school, but it should be taught from a clinical standpoint. Every city in which there are medical schools should have a psychopathic hospital for the reception of all cases of alleged insanity and for their study, treatment and cure. Such a hospital should contain, also, a laboratory for the study of normal and of pathological psychology. I am convinced that progress in normal psychology will be made chiefly through the study of abnormal conditions, just as physiology has profited so enormously through the work of the pathologist."

A word should be said for veterinary medicine and its achievements of enormous economic value in the control of the contagious diseases of animals. The recent achievements of vaccination against the southern cattle fever and against tuberculosis, the eradication of the foot and mouth disease among other matters, have demanded the highest scientific knowledge and the greatest skill in its practical application.

Unfortunately, veterinary science lacks in this country adequate facilities for research and instruction. "Practically," says a correspondent, "the veterinary sciences in the United States are leading a parasitic existence. We are dependent almost wholly upon the results of investigation and teaching of European countries, notably Germany and Denmark. The value of the live-stock industry here is so tremendous that almost every state in the Union should have a well-equipped veterinary school supported by public funds. There is but one veterinary school in the United States that has anything like adequate support." That this is true shows that our farmers and stock-raisers are very far from having an adequate idea of one of the most important of their economic needs.

Economics.

We may justify the inclusion of economics among the utilitarian sciences on grounds which would equally include the sciences of ethics and hygiene. It is extremely wise as well as financially profitable to take care of one's health, and still more so to take thought of one's conduct. The science of economics in some degree touches the ethics of nations and the 'wealth of nations,' a large factor in the happiness of the individuals contained within them, depends on the nation's attitude towards economic truths. Another justification of this inclusion is found in the growing tendency in our country to call on professional

economists to direct national operations. On the other hand, our economists themselves are becoming more and more worthy of such trusts. The inductive study of their science brings them into closer contact with men and with enterprises. By this means they become students of administration as well as of economics. They realize the value of individual effort as well as the limitations which bound all sorts of executive work, in a republic. "Only a few years ago," writes a correspondent, "the teachers of economics were far more generally unfavorable critics of government work which interested them. They have become more and more disposed to cooperate at the beginning rather than to condemn at the end. Just as economics has taken a more kindly and hospitable attitude towards politics, so similarly has it towards business, as illustrated in the rapid rise of courses in commerce." The demand for trained economists in public affairs is 'compelling the teachers of economics more and more to seek contact with the men who are grappling face to face with economic problems.'

The relation of economic theory to administration is a subject on which there is much diversity of opinion. It is claimed by able authority that "Economic science, by becoming ultra-theoretical, has come into far closer touch with practical life than it ever attained before. Laws, the statement of which seems like a refinement of theory, determine the kind of legislation required on the most practical of subjects." On another hand, it is claimed by high authority that our country must have its own political economy. "The generalizations arising solely from the uniformity of human nature are so few that they can not constitute a science. The classical or orthodox Political Economy of England was conditioned from start to finish by the political problems it had to face. We are only beginning to acquire our national independence."

Still another view is that "all that has been achieved in the field of economics that is of any value, has been the result of logical analysis applied to the phenomena and experiences of every-day industrial life. The stages of past development can be determined and interpreted only in the light of this analysis. The lesson which the historical economist has never learned, is the importance of that principle, which lies at the bottom of the whole modern theory of evolution, and which was made use of by Lyell and Darwin, namely, the principle that historical changes of the past are to be accounted for by the long continued action of causes which are at this present moment in operation and can be observed and measured at the present day." "This," says my correspondent, "needs saying and resaying, until it is burned into the minds of all students of economics."

The recent progress of economics in America has lain in part in the development of economic theory by critical and by constructive methods. An important reason for welcoming the exact and critical study of economic theory is this: In the promulgation of imaginary

economic principles the social and political charlatan finds his choice field of operation, just as the medical charlatan deals with some universal law of disease and its universal cure. The progress of science in every field discredits these universal principles with their mystical panaceas. There is all the more reason why in politics, as in medicine, those generalizations which deal with necessary laws or actually observed sequence of events should be critically and constructively studied.

In general, however, the progress of economics has followed the same lines as progress in other sciences, through a 'minute investigation and the application of principles already discovered or outlined by painstaking inquiry as to facts.' This method of work has been especially fruitful in the study of monetary problems, of finance, taxation and insurance, in the study of labor problems and conditions, in the study of commerce and in the study of crime and pauperism. In its development economics is, however, many years behind the natural sciences, a condition due to reliance on metaphysical methods and to the inherent difficulty in the use of any other.

"Economics," says a correspondent, "has been less successful than the material sciences in getting rid of the apparatus of metaphysical presumptions. The economist is still too eager to formulate laws that shall disclose the ultimate spiritual meaning of things instead of trying to explain how these things came to pass. He has profited in small degree by those lessons which the progressive evolutionary sciences have driven home in the past, in the methods of thinking of workers in other fields. Our science is still sadly behind the times in its way of handling its subject matter. The greatest and most important work of economic investigations is to make students see things as they are, to fit young men for the more highly organized business new conditions are ushering in, and give a better appreciation of the problems of government and a better training for participation in them."

Says another correspondent: "Training in research is in fact essential to every technical man. The young technologist will be confronted by new problems not covered by anything in literature or in his past experience. Training in research is training in the art of solving unsolved problems, and the practical man who has had discipline of that kind has a great advantage over his more conventional competitors. The Germans recognize this principle, and behold their marvelous industrial growth. The student in every department of science should be taught to think as well as to do."

The time must come when a man who has no training and no experience in research will not be called educated, whatever may be the range of his erudition. To unfold the secret of power is the true purpose of education.

THE EVOLUTION OF THE SCIENTIFIC INVESTIGATOR.*

By Professor SIMON NEWCOMB, U. S. N. (retired),
WASHINGTON, D. C.

AMONG the tendencies characteristic of the science of our day is one toward laying greater stress on questions of the beginning of things, and regarding a knowledge of the laws of development of any object of study as necessary to its complete understanding in the form in which we find it. It may be conceded that the principle here involved is as applicable in the broadest field of thought as in a special research into the properties of the minutest organism. It, therefore, seems meet that the comprehensive survey of the realm of knowledge on which we are about to enter should begin by seeking to bring to light those agencies which have brought about the remarkable development of that realm to which the world of to-day bears witness. The principle in question is recognized in the plan of our proceedings by providing for each great department of knowledge a review of its progress during the century that has elapsed since the great event which the scene around us is intended to commemorate. But such reviews do not make up that general survey of science at large which is necessary to the development of our theme, and which must include the action of causes that had their origin long before our time. The movement which culminated in making the nineteenth century ever memorable in history is the outcome of a long series of causes, acting through many centuries, which are worthy of being brought into especial prominence on such an occasion as this. In setting them forth we should avoid laying stress on those visible manifestations which, striking the eye of every beholder, are in no danger of being overlooked, and search rather for those agencies whose activities underlie the whole visible scene, but which are liable to be blotted out of sight by the very brilliancy of the results to which they have given rise. It is easy to draw attention to the wonderful qualities of the oak; but, from that very fact, it may be needful to point out that the real wonder lies concealed in the acorn from which it grew.

Our inquiry into the logical order of the causes which have made our civilization what it is to-day will be facilitated by bringing to mind certain elementary considerations—ideas so familiar that setting them forth may seem like citing a body of truisms—and yet so frequently

* Opening and concluding parts of the address of the president of the International Congress of Arts and Science, at the St. Louis Exposition, September 19, 1904.

overlooked not only individually, but in their relation to each other, that the conclusion to which they lead may be lost to sight. One of these propositions is that psychical rather than material causes are those which we should regard as fundamental in directing the development of the social organism. The human intellect is the really active agent in every branch of endeavor—the *primum mobile* of civilization —and all those material manifestations to which our attention is so often directed are to be regarded as secondary to this first agency. If it be true that 'in the world is nothing great but man; in man is nothing great but mind,' then should the keynote of our discourse be the recognition at every step of this first and greatest of powers.

Another well-known fact is that those applications of the forces of nature to the promotion of human welfare which have made our age what it is are of such comparatively recent origin that we need go back only a single century to antedate their most important features, and scarcely more than four centuries to find their beginning. It follows that the subject of our inquiry should be the commencement, not many centuries ago, of a certain new form of intellectual activity.

Having gained this point of view our next inquiry will be into the nature of that activity, and its relation to the stages of progress which preceded and followed its beginning. The superficial observer, who sees the oak but forgets the acorn, might tell us that the special qualities which have produced such great results are expert scientific knowledge and rare ingenuity, directed to the application of the powers of steam and electricity. From this point of view the great inventors and the great captains of industry were the first agents in bringing about the modern era. But the more careful inquirer will see that the work of these men was possible only through a knowledge of laws of nature which had been gained by men whose work took precedence of theirs in logical order, and that success in invention has been measured by completeness in such knowledge. While giving all due honor to the great inventors, let us remember that the first place is that of the great investigators whose forceful intellects opened the way to secrets previously hidden from men. Let it be an honor and not a reproach to these men that they were not actuated by the love of gain, and did not keep utilitarian ends in view in the pursuit of their researches. If it seems that in neglecting such ends they were leaving undone the most important part of their work, let us remember that nature turns a forbidding face to those who pay her court with the hope of gain, and is responsive only to those suitors whose love for her is pure and undefiled. Not only is the special genius required in the investigator not that generally best adapted to applying the discoveries which he makes, but the result of his having sordid ends in view would be to narrow the field of his efforts, and exercise a depressing effect upon his activities. It is impossible to know what application knowledge

may have until after it is acquired, and the seeker after purely useful knowledge will fail to acquire any real knowledge whatever.

We have here the explanation of the well-known fact that the functions of the investigator of the laws of nature, and of the inventor who applies these laws to utilitarian purposes are rarely united in the same person. If the one conspicuous exception which the past century presents to this rule is not unique, we should probably have to go back to Watt to find another. The true man of science of to-day and of all past time has no such expression in his vocabulary as useful knowledge. His domain is the whole of nature, and were he to attempt its division into the useful and the useless, he would drop from his high estate.

It is, therefore, clear that the primary agent in the movement which has elevated man to the masterful position he now occupies is the scientific investigator. He it is whose work has deprived plague and pestilence of their terrors, alleviated human suffering, girdled the earth with the electric wire, bound the continent with the iron way, and made neighbors of the most distant nations. As the first agent which has made possible this meeting of his representatives, let his evolution be this day our worthy theme.

It has been said that the scientific investigator is a new species of the human race. If this designation is applicable to a class defined only by its functions, then it is eminently appropriate. But the biologist may object to it on the ground that a species, or even a variety, is the product of heredity, and propagates only or mainly its own kind. The evolutionist may join hands with him on the ground that only new faculties, not new modes of activity, are to be regarded as products of evolution, but let us not stop to dispute about words. We have no need of the term 'species' in our present course of thought; but to deny the term evolution to the genesis of previously non-existent forms of intellectual activity is to narrow our conception of the course of nature, and draw a line of demarkation where no tangible boundary exists.

I am the more ready to invite your attention to the evolution of the scientific investigator, not only because the subject is closely correlated with human evolution in general, but because it is one branch of evolution which seems to me not to have received due prominence in discussions of the subject.

* * * * * *

There is an increasing recognition of methods of research and of deduction which are common to large branches or to the whole of science. We are more and more recognizing the principle that progress in knowledge implies its reduction to a more exact form, and the expression of its ideas in language more or less mathematical. The problem before the organizers of this congress was, therefore, to bring the sciences together, and seek for the unity which we believe underlies

their infinite diversity. The assembling of such a body as now fills this hall was scarcely possible in any preceding generation, and is made possible now only through the agency of science itself. It differs from all preceding international meetings by the universality of its scope, which aims to include the whole of knowledge. It is also unique in that none but leaders have been sought out as members. It is unique in that so many lands have delegated their choicest intellects to carry on its work. They come from the country to which our republic is indebted for a third of its territory, including the ground on which we stand; from the land which has taught us that the most scholarly devotion to the languages and learning of the cloistered past is compatible with leadership in the practicable application of modern science to the arts of life; from the island whose language and literature have found a new field and a vigorous growth in this region; from the last seat of the holy Roman Empire; from the country which, boasting of the only monarch that ever made an astronomical observation at the Greenwich Observatory, has enthroned science in one of the highest places in its government; from the peninsula so learned that we have invited one of its scholars to come here and teach us our own language; from the land which gave birth to Leonardo, Galileo, Torricelli, Columbus, Volta—what an array of immortal names!—from the little republic of glorious history which, breeding men rugged as its eternal snow-peaks, has yet been the seat of scientific investigation since the day of the Bernoullis; from the land whose heroic dwellers did not hesitate to use the ocean itself to protect it against invaders, and which now makes us marvel at the amount of erudition compressed within its little area; from the nation of the farthest east, which, by half a century of unequaled progress in the arts of life, has made an important contribution to evolutionary science through demonstrating the falsity of the theory that the most ancient races are doomed to be left in the rear of the advancing age—in a word, from every great center of intellectual activity on the globe I see before me eminent representatives of that world-advance which we have come to celebrate.

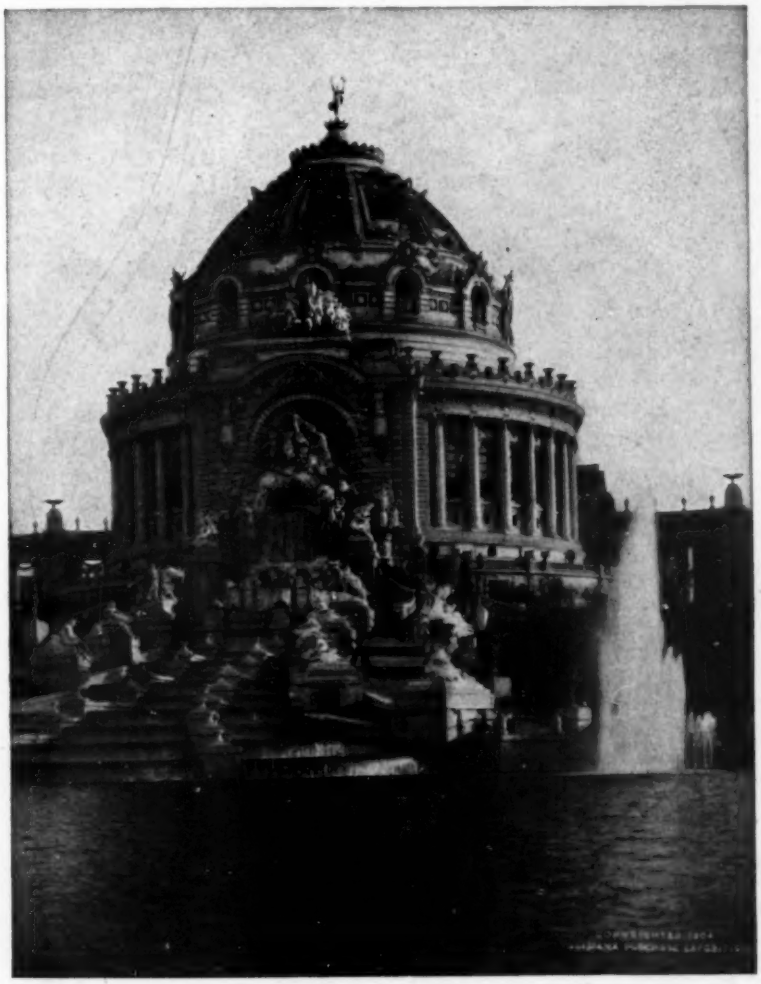
Gentlemen and scholars all! You do not visit our shores to find great collections in which long centuries of humanity have given expression on canvas and in marble to their hopes, fears and aspirations. Nor do you expect institutions and buildings hoary with age. But as you feel the vigor latent in the fresh air of these expansive prairies, which has collected the products of human genius by which we are here surrounded and, I may add, brought us together—as you study the institutions which we have founded for the benefit not only of our own people but of humanity at large; as you meet the men who, in the short space of one century, have transformed this valley from a savage wilderness into what it is to-day—then may you find compensation for the want of a past like yours by seeing with prophetic

eye a future world power of which this region shall be the seat. If such is to be the outcome of the institutions which we are now building up, then may your present visit be a blessing both to your posterity and ours, by making that power one for good to all mankind. Your deliberation will help to demonstrate to us and to the world at large that the reign of law must supplant that of brute force in the relations of the nations, just as it has supplanted it in the relations of individuals. You will help to show that the war which science is now waging against the sources of disease, pain and misery offers an even nobler field for the exercise of heroic qualities than can that of battle. We hope that when, after your all too fleeting sojourn in our midst, you return to your own shores, you will long feel the influence of the new air you have breathed in an infusion of increased vigor in pursuing your varied labors. And if a new impetus is thus given to the great intellectual movement of the past century, resulting not only in promoting the unification of knowledge, but in widening its field through new combinations of effort on the part of its votaries, the projectors, organizers and supporters of this Congress of Arts and Science will be justified of their labors.

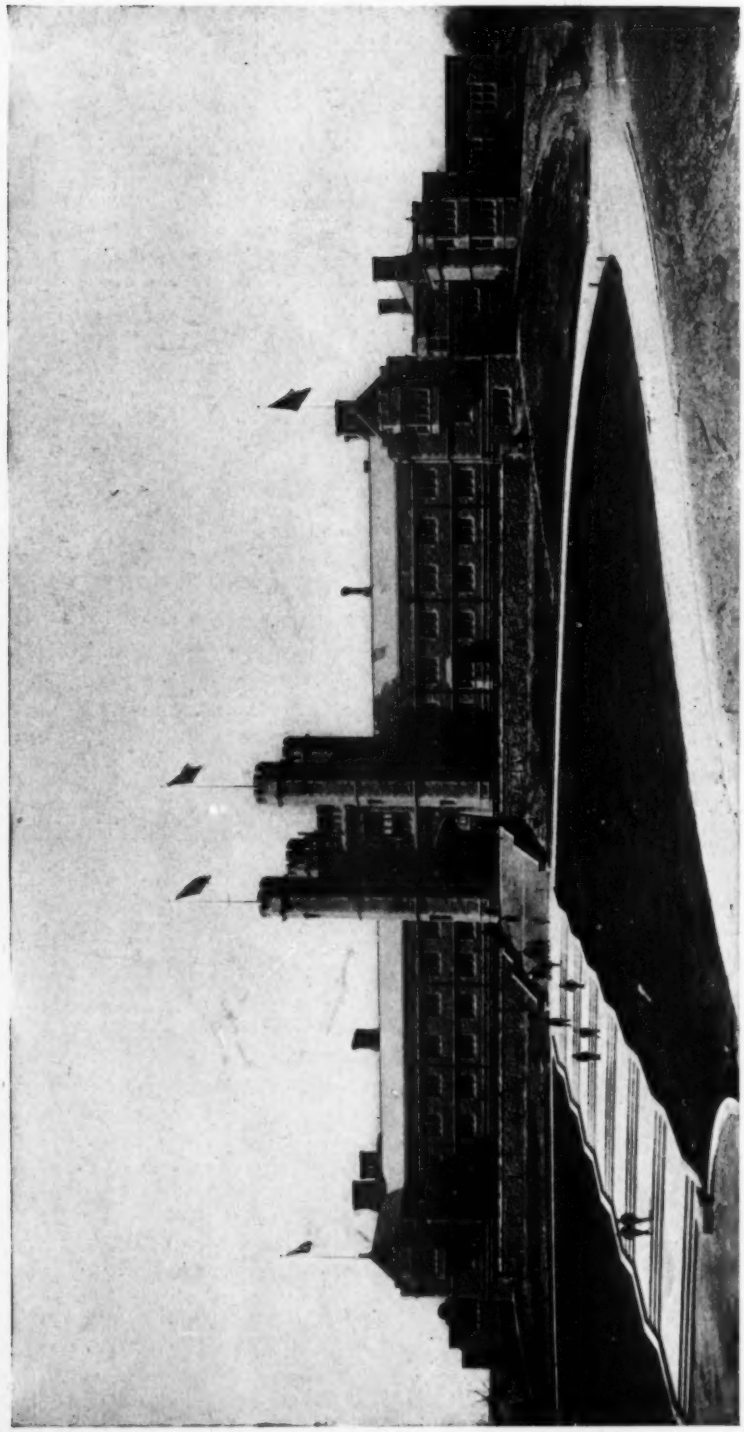
THE PROGRESS OF SCIENCE.

THE INTERNATIONAL CONGRESS OF ARTS AND SCIENCE.

THIS number of THE POPULAR SCIENCE MONTHLY is given to the St. Louis Congress of Arts and Science, which is certainly an event in the history of science deserving special commemoration. Never before in the world have there come together a group of scholars whose average performance is so great, and it is a matter for legimate national pride that this assemblage should have gathered in the United States. The objects of the congress were described by Professor Newcomb, the president, in the September issue of the *Monthly*, and there is given above



FESTIVAL HALL, IN WHICH THE OPENING EXERCISES OF THE INTERNATIONAL CONGRESS OF ARTS AND SCIENCE WERE HELD.



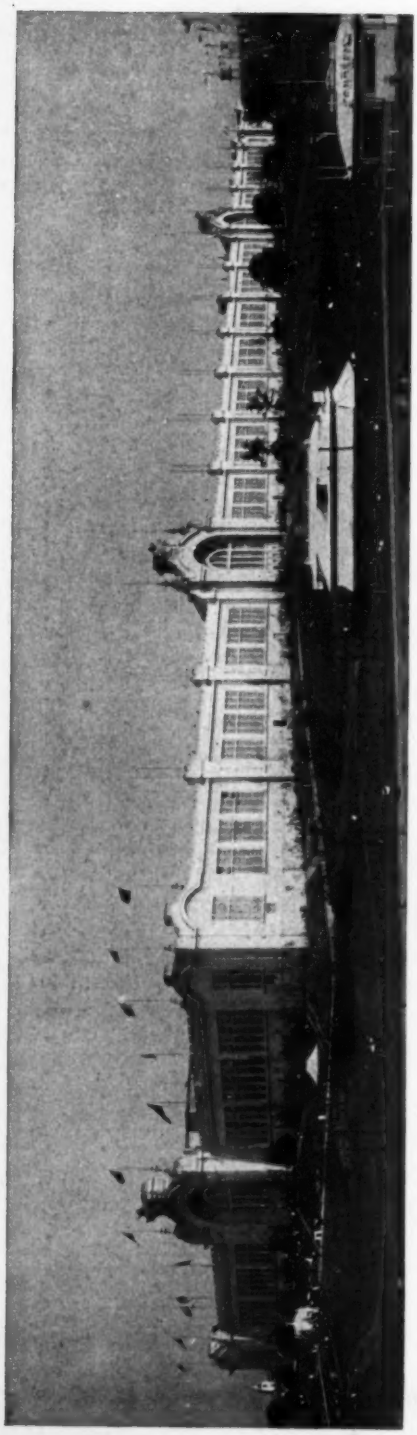
ADMINISTRATION BUILDING, BEING PART OF WASHINGTON UNIVERSITY, WHICH WAS THE HEADQUARTERS OF THE INTERNATIONAL CONGRESS OF ARTS AND SCIENCE.

PALACE OF EDUCATION AND SOCIAL ECONOMY.

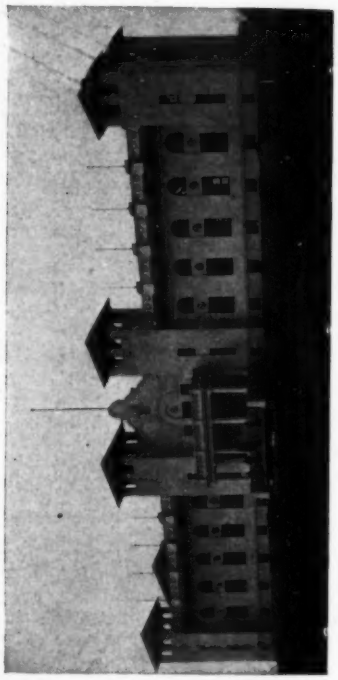
an appreciation of its work by Mr. Wm. Harper Davis, of Lehigh University, one of the secretaries. The articles following are addresses given at the congress, which have not been published elsewhere.

International congresses have gradually come to be a part of international expositions. At Paris over a hundred congresses were held, extending through the summer, and the sessions and the subsequent publication of the proceedings form an important chapter in the history of modern science. When the managers of the St. Louis exposition decided to make international congresses a part of their scheme, they appointed a representative administrative board, with President Nicholas Murray Butler, of Columbia University, as chairman. This board adopted the plan proposed by Professor Münsterberg, of Harvard University, to hold one congress of the arts and sciences which should attempt to promote and demonstrate the unity of science. Professor Newcomb was appointed president, and Professors Münsterberg and Small, vice-presidents, and at the same time acted as a committee of organization.

All this is, however, told in Mr. Davis's article. After giving the praise and appreciation that is due, it may be well to call atention to some of the lessons of the congress. It was a fine idea to have the whole range of modern civilization represented in a great international gathering of the leaders in all departments of the sci-



PALACE OF AGRICULTURE, WHICH COVERS TWENTY ACRES.



INDIAN SCHOOL.

FISHERIES BUILDING.

ences and the arts. The attempt to unify knowledge on the lines of a particular philosophical system must naturally fail. No one supposes that a conference at the Hague will give the world enduring peace, or that a congress at St. Louis will unify the sciences. Indeed there are those who hold that science will be unified only when it is dead, and that any scheme of unification is more useful in promoting controversy than in prescribing a final solution. It is probable that very few of the speakers were even aware of Professor Münsterberg's plan or had read his article in the *Atlantic Monthly*. The addresses that dealt with some special problem to which the author had contributed were the best. The divisions intended to unify the sciences were superfluous. As a matter of fact it is more feasible and more profitable to unify men of science than to perfect a logical scheme of the sciences, and in this direction the congress was only moderately successful.

Incidentally much was indeed accomplished, the main result being the bringing of a hundred leading foreign scholars to this country. Not only at the congress, but in their visits to other places, they have taught us many things, and it may be hoped have learned some things from us. The two hundred thousand dollars expended is a considerable sum, and possibly still more might have been accomplished with it. It is doubtful whether the limitation of the meeting to a single week represents any advance over the series of congresses of the Paris exposition. If the dormitories of Washingeon University, with a proper dining-room and rooms for seasions and social intercourse, had been placed at the disposal of our national societies, and they had held a series of meetings during the summer, with perhaps one week for general addresses by a score of invited scholars, the results would probably have been better.

As it was the week of the congress was overcrowded. Each of the some three hundred speakers addressed an

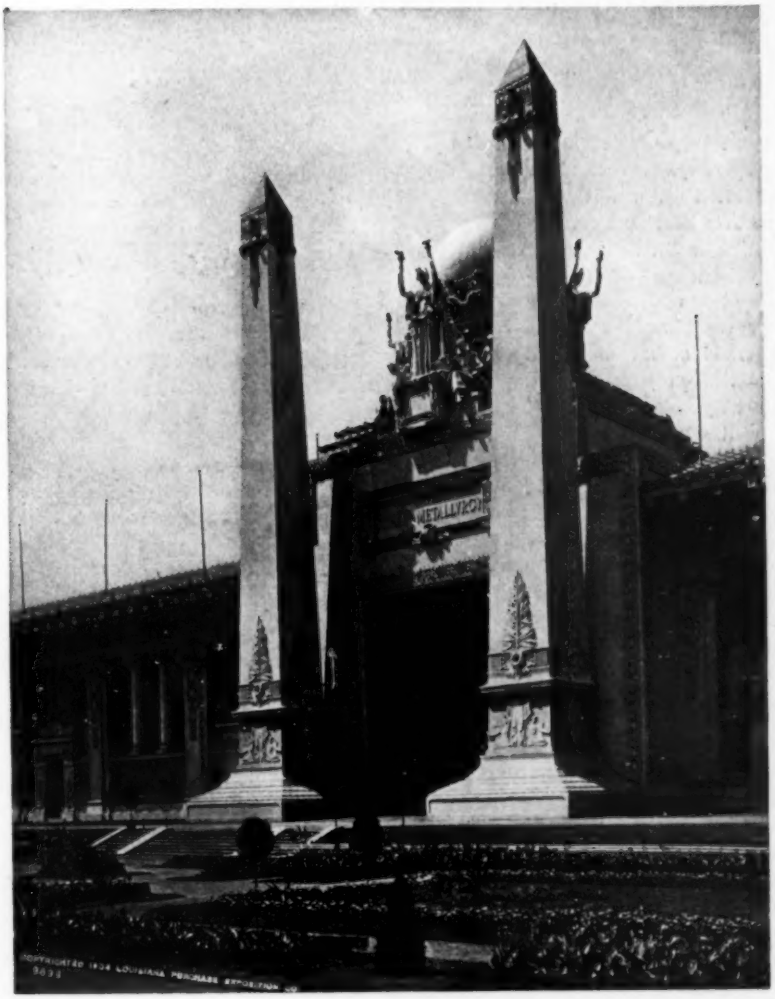
PALACE OF MACHINERY.

PALACE OF ELECTRICITY.

audience which probably averaged about fifty, consisting in part of specialists and in part of chance visitors. It was but seldom that students of one science listened to speakers in another, and the opportunities to become personally acquainted were inadequate. Officers of the army of science were paid to be present, but the rank and file of American workers were not there. And this was largely the fault of defective organization. We have, apart from the national scientific societies and local academies, at least four institutions which should have worked in harmony with the congress—the American Association for the Advancement of Science, the National Academy of Sciences, the Smithsonian Institution and the Carnegie Institution. But the cooperation of none of these bodies was secured, the head of none of them was present at St. Louis. They even worked at cross purposes, the American Association having met at St. Louis last Christmas and the National Academy having met at Chicago in November. An attempt to unify science which made no use of existing organizations was seriously at fault. The management also failed to bring science and scholarship into con-

tact with the wider public, the congress passing almost unnoticed, except by those immediately concerned.

But it may seem ungracious to call attention to what was not done when so much was in fact accomplished. The congress was worth many fold the efforts and the money that it cost. For the first time in the history of the world the attempt was made to bring together the leaders in all departments of science, scholarship and the arts. The large plan will ultimately emerge from the mass of details and will be a landmark in the history of civilization.



PALACE OF MINES AND METALLURGY.

THE LOUISIANA PURCHASE EXPOSITION.

AN international exposition offers certain advantages and certain drawbacks as a place of meeting for an international congress. The drawbacks —both physical and mental—are sufficiently obvious. They are perhaps given more weight than they deserve, and thus a new obstacle is set up. With a comparatively small change in the conditions that have ordinarily prevailed, an exposition and a congress should help each other. They have in many ways the same ends in view. As civilization advances science and the arts become an increasing part of life. The St. Louis exposition, all the way from the Pike to the International Congress of Arts and Science, overlaps continually with the objects and the field of this magazine. It would be satisfactory if we could give an adequate appreciation and criticism of the exposition, but this does not appear to be feasible. It may, however, be worth the while in a number of the MONTHLY devoted to the International Congress to give some illustrations showing its material setting, and to devote a few words to the exposition itself.

The magnitude of the exposition, its hundreds of buildings, measured by the acre, and the tens of millions of dollars that it cost have been duly advertised; and allowance is made by sensible people for crudeness, flimsiness and heterogeneity. A certain architectural unity has been given to the whole scheme by the fan-like radiating avenues or plazas which converge towards the central festival hall. At night, under the electric illumination, the effects are marvelous and beautiful. It can scarcely be claimed, however, that any significant advance has been made beyond the Chicago exposition. It seems that the limits of magnitude and universality have been reached, and that subsequent international expositions, should they occur, must aim to surpass their predecessors in completeness in some particular direction.

In the classification of the St. Louis Exposition education was given the central place, and the fact that the new buildings of Washingeon University were occupied also emphasized higher education. Germany made a fine educational exhibit, and an Indian school and other schools were shown in operation. It would have been well if the buildings of Washington University could have been used to show a national or international university in operation with the speakers of the International Congress as the teachers; but this would doubtless be asking too much. Anthropology, directly and indirectly, should occupy a prominent place in an international exposition. At St. Louis the Philippine exhibit was timely and well arranged, with its native villages and the thousand representatives of the different peoples.

The progress of the applied sciences since the Chicago exposition is doubtless the most notable feature of the period, and this was adequately represented at St. Louis. The names of the buildings—agriculture, machinery, electricity, mines and metallurgy, etc.—make it clear that an exposition is practically an exhibit of applied science. The advances in America during this period have probably been unsurpassed, but the exhibits of Germany and Japan show that they are not unrivaled.